UNIVERSITÀ DEGLI STUDI DI PADOVA

Sede Amministrativa: Università degli Studi di Padova

Dipartimento di Scienze Economiche e Aziendali "Marco Fanno"

SCUOLA DI DOTTORATO DI RICERCA IN

ECONOMIA E MANAGEMENT

CICLO XXVI

ESSAYS IN LABOR ECONOMICS: CROSS-COUNTRY EVIDENCE

Direttore della Scuola: Ch.mo Prof. Giorgio Brunello

Supervisore: Ch.mo Prof. Guglielmo Weber

Dottorando: Luca Stella

Luca Stella

UNIVERSITY OF PADUA

DEPARTMENT OF ECONOMICS

Dissertation

ESSAYS IN LABOR ECONOMICS: CROSS-COUNTRY EVIDENCE

by

LUCA STELLA

Doctor of Philosophy

2014

Contents

Introduction

This doctoral thesis consists of three empirical research papers, in which intergenerational links and their effects on economic outcomes are investigated.

The first paper, titled "Intergenerational Transmission of Human Capital in Europe: Evidence from SHARE", extends the previous literature on the intergenerational transmission of human capital by exploiting variation in compulsory schooling reforms across nine European countries over the period 1920-1956. My empirical strategy follows an instrumental variable (IV) approach, instrumenting parental education with years of compulsory schooling. I find some evidence of a causal relationship between parents' and children's education. The magnitude of the estimated effect is large: an additional year of parental education raises the child's education by 0.44 of a year. I also find that maternal schooling is more important than paternal schooling for the academic performance of their offspring. The results are robust to several specification checks.

The second paper, titled "Living Arrangements in Europe: Whether and Why Paternal Retirement Matters", uses retrospective micro data from eleven European countries to investigate the role of paternal retirement in explaining children's decisions to leave the parental home. To assess causality, I use a bivariate discrete hazard model with shared frailty and exploit over time and cross-country variation in early retirement legislation. Overall, the results indicate a positive and significant influence of paternal retirement on the probability of first nest-leaving of children residing in southern European countries, both for sons and daughters. By contrast, there is no evidence of significant effects on children living in northern and central European countries. I then discuss the potential mechanisms by which paternal retirement may affect children's nest-leaving. I find that the increase in children's nest-leaving around paternal retirement does not seem to be driven by changes in parents' budget constraints. Rather, one must probably look for channels involving negative externalities in preferences between parents and children.

Finally, the third paper, titled "The Effect of Divorce Risk on the Wealth and Retirement Security of Households", quantifies the effects of divorce risk on couple's retirement well-being in Europe using changes in divorce laws occurred between the late 1970s and the 2000s. Across countries and over time, the ground for divorce shifted from mutual consent to unilateral choice. This "divorce revolution" allows for the use of a quasi-experimental design that exploits the time and country variation in these laws to identify the empirical relationship between divorce risk and couple's economic security in retirement. By employing a unique dataset that contains complete marital history for different European countries, combined with features of divorce legislation across countries and over time, I can quantify their effects on the cohort of married couples born over the period 1920-1957.

Introduzione

Il presente lavoro e' costituito da tre articoli accademici, di natura empirica, focalizzati sul tema dei legami intergenerazionali e sui loro effetti su una serie di economic outcomes.

Il primo capitolo, "Intergenerational Transmission of Human Capital in Europe: Evidence from SHARE", estende la precedente letteratura sul meccanismo intergenerazionale di trasmissione di capitale umano sfruttando la variazione nelle leggi di istruzione obbligatoria in nove Paesi Europei tra il 1920 e il 1956. La strategia empirica e' basata su un approccio a variabili strumentali, in cui l'istruzione dei genitori viene strumentata utilizzando gli anni di istruzione obbligatoria. Trovo evidenza di una relazione causale tra istruzione dei genitori e istruzione dei figli. L'ampiezza del coefficiente stimato e' grande: un anno addizionale nell'istruzione dei genitori aumenta l'istruzione dei figli di 0.44 anni. Trovo inoltre evidenza che l'istruzione materna e' piu' importante dell'istruzione paterna sulla performance accademica dei figli. I risultati sono robusti rispetto ad una serie di robustness checks.

Il secondo paper, "Living Arrangements in Europe: Whether and Why Paternal Retirement Matters", usa dati retrospettivi di undici Paesi Europei con l'obiettivo di studiare il ruolo del pensionamento del padre sulla decisione dei figli di lasciare la casa dei genitori. Per quanto riguarda l'identificazione, uso un modello hazard bivariato con shared frailty e sfrutto la variazione over time e cross-country nelle riforme pensionistiche. Nel complesso, i risultati indicano un effetto positivo e significativo da parte del pensionamento del padre sulla probabilita' che i figli escano di casa per la prima volta nei Paesi del Sud Europa., sia per i figli maschi sia per le figlie femmine. Al contrario, non c'e' evidenza di effetti significativi per i figli che vivono nei Paesi del Nord e del Centro Europa. Discuto poi i meccanismi tramite cui il pensionamento del padre puo' influenzare l'uscita di casa dei figli. Trovo che l'aumento nell'uscita di casa dei figli al momento del pensionamento del padre non sembra essere guidato da modifiche nel vincolo di bilancio dei genitori. Piuttosto, uno dovrebbe probabilmente guardare ai canali che riguardano le esternalita' negative nelle preferenze tra genitori e figli.

Infine, il terzo paper, "The Effect of Divorce Risk on the Wealth and Retirement Security of Households", quantifica gli effetti associati al rischio di divorzio sul retirement well-being della coppia in Europa usando le leggi sul divorzio avvenute tra la fine del 1970 e gli anni 2000. La ragione del divorzio e' passata da mutual consent a unilateral choice. Questa "divorce revolution" permette quindi di ricorrere ad un quasi-experimental design che sfrutta variazione over time e cross-country per studiare la relazione empirica tra rischio di divorzio e il retirement security della coppia. Utilizzando un dataset che contiene informazione sulla marital history per differenti Paesi Europei, e sfruttando le leggi sul divorzio over time e cross-country, sono in grado di quantificare il loro effetto sulle coorte delle coppie nate tra il 1920 e il 1957.

Chapter 1

Intergenerational Transmission of Human Capital in Europe: Evidence from SHARE

Abstract: This paper extends the previous literature on the intergenerational transmission of human capital by exploiting variation in compulsory schooling reforms across nine European countries over the period 1920-1956. My empirical strategy follows an instrumental variable (IV) approach, instrumenting parental education with years of compulsory schooling. I find some evidence of a causal relationship between parents' and children's education. The magnitude of the estimated effect is large: an additional year of parental education raises the child's education by 0.44 of a year. I also find that maternal schooling is more important than paternal schooling for the academic performance of their offspring. The results are robust to several specification checks.

1.1 Introduction

The notion that there is a positive association between the educational outcomes of the parents and their children is well documented. However, while there is a substantial consensus on this intergenerational correlation, less is known about the existence of a causal relationship underlying the transmission of education between generations (see, for instance, Black et al. 2005; Oreopoulos et al. 2006; Bjorklund and Salvanes 2010).

On the policy side, to the extent that policymakers are concerned about early school leavers, an analysis of the mechanisms through which education is passed on from parents to children is particularly relevant in light of reforms that extend the length of compulsory schooling. For example, if there is evidence that parental education is responsible for children's performance in school, then interventions that improve the educational attainment of less educated parents should lead to increased human capital among their children, thus reducing the degree of inequality of opportunity in education.

However, the primary concern is that intergenerational educational estimates might not adequately account for the correlation between parental schooling and some unobserved, inherited characteristics that might affect the academic achievement of their offspring. Such correlations would imply that the intergenerational transmission of education could be primarily driven by selection rather than reflecting a causal relationship running from a parent's to a child's education. To address this concern regarding endogeneity caused by omitted variables, the empirical literature has recently focused on three identification strategies: twin parents (Behrman and Rosenzweig 2002); adopted children (Plug 2004; Björklund et al. 2006); and instrumental variables (Black et al. 2005; Oreopoulos et al. 2006).

In my study, I employ this latter IV approach that obtains identification from compulsory schooling laws that influence the educational distribution of the parents without directly affecting the children. In particular, this study is strictly connected to the seminal paper by Black et al. (2005), which, using the Norwegian schooling reforms during the sixties and early seventies, finds no evidence of a causal impact of parental education on the next generation's education, with the exception of the weak impact of maternal schooling on educational attainment among sons. Similarly, Holmlund et al. (2011), applying this methodology to Sweden, obtain results in line with Black et. al. (2005). However, these findings of limited effects of parental education in Norway and Sweden have not been supported by studies for other countries (see, for example, Oreopoulos et al. 2006 for the USA; Chevalier 2004 for the UK; and Maurin and McNally 2008 for France). Perhaps these contradictory results are related to the relatively low levels of inequality with respect to economic and educational outcomes in Scandinavian countries.

The contribution of this paper to the literature is twofold. To my knowledge, there are no studies that examine the causal effect of parental schooling on the human capital of their children by exploiting the variation provided by compulsory schooling laws over time and across European countries. Therefore, this paper adds to previous research by using this source of exogenous variation in parental schooling to disentangle the direction of causality. Another contribution of this paper is to shed new light on the different roles played by mothers and fathers in explaining the transmission of education to their sons and daughters. The findings from this multi-country analysis contribute to our understanding of how and why education is transmitted across generations by accounting for the effects of different institutional and cultural environments in Europe. A key element of my identification strategy is that it makes it possible to control for both country fixed effects, which account for time-invariant characteristics across countries, and birth cohort fixed effects for parents, which will capture any systematic difference in schooling outcomes across parental cohorts. To conduct this analysis, I draw data from the first two waves (2004 and 2006) of the Survey of Health Ageing and Retirement in Europe (SHARE). This European dataset has three important features: first, it collects data on the current economic, health, and family conditions of over 30,000 individuals aged fifty and above in several European countries; second, it provides information on educational attainment for two family generations; and finally, as it is designed to be cross-nationally comparable, this dataset enables me to properly conduct a multi-country analysis. Furthermore, I use data on reforms of the minimum school leaving age by relying on recent studies (Brunello et al. 2009; Brunello et al. 2012; Garrouste 2010).

Based on these data, my main results demonstrate that: a) when omitting country-specific trends, there is some evidence of a causal relationship between parents' and children's education. The magnitude of the effect is large: an additional year of parental education induced by the reform generates 0.44 years of additional schooling for their children; b) when including country-specific trends, the estimated effects of parental education are no longer statistically significant. I argue that this lack of statistical significance can be explained by the fact that the addition of country-specific trends greatly reduces the first stage power of my instrument; c) the mother's schooling has a slightly stronger impact than that of her husband on the academic achievement of their offspring with or without country-specific trends. These findings are robust to a number of specification checks.

The remainder of the paper is organized as follows. The next section discusses the relevant literature on the intergenerational transmission of education. Section 3 presents a description of the data and illustrates the main features of European compulsory schooling reforms. Section 4 describes the empirical specification and identification strategy. The main results of the paper are presented in Section 5, and Section 6 provides robustness checks. I discuss the results in Section 7. Concluding remarks are provided in Section 8.

1.2 Literature Review

Over the last decade, several empirical studies have attempted to shed some light on the causal mechanism that underlies the relationship between parents' and children's educational outcomes. These studies have proposed different strategies to identify exogenous variation in parental schooling. In the literature to date, there are three main research streams investigating the causal effect of parental education on their offspring's education. These streams differ in their choice of identification strategy. Below, I present a brief review of these studies and explain my contribution relative to the existing literature.[1]

The first strand of the literature examines the causal relationship between parental and children's education using data on pairs of identical twin parents to difference out not only family fixed effects but also unobserved factors due to the parents' genetics. One of the first studies, by Behrman and Rosenzweig (2002), compares the schooling of the children of twin mothers and twin fathers who were identical in all characteristics except their level of educational attainment. While Behrman and Rosenzweig's findings suggest a

[1]A more detailed summary of the literature on each identification strategy may be found in Holmlund et al. (2008); Bjorklund and Salvanes (2010); and Black and Devereux (2010). In particular, Holmlund et al. (2008) argue that the conflicting results across these three literatures arise mostly from the different identification strategies rather than from differences in the countries that have been studied.

positive and large effect of the father's schooling but no effect from the mother's schooling, Antonovics and Goldberger (2005) questione the validity of these results by demonstrating their sensitivity to school coding schemes and sample selection rules.

The second stream of the literature estimates intergenerational schooling effects using samples of parents and their adopted children. Sacerdote (2002) and Plug (2004) compare adopted and natural children and conclude that environmental factors are important for the intergenerational transmission of education. However, these studies were severely limited by the paucity of data on the adopted children and a lack of information on the biological parents of adoptees. To overcome these issues, the literature has recently made use of large registry datasets of adopted children, which are available in the Nordic countries. In their study, Björklund et al. (2006) improve on the previous literature by employing a unique administrative dataset of Swedish adoptees that allowed them to examine the impact of both the adoptive and biological parents' years of schooling on the adopted child's years of schooling. They find both the adoptive and the biological parents' education to be important. Overall, studies on adopted children emphasize the importance of both genetic and environmental factors for a child's success in school.

Finally, there is a strand of the literature based on instrumental variables. This IV approach is the one I apply in this paper, and is closely related to the seminal paper by Black et al. (2005), which utilizes the Norwegian schooling reforms that occurred in different municipalities for the period 1959-1973. This study provides little evidence for the causal effects of parental education on children's attainment. Overall, the authors conclude that the father's schooling has no impact on children's educational attainment despite a positive, but small, intergenerational effect between mothers and their sons. Similar results were obtained in a more recent paper by Holmlund et al. (2008) applying the same strategy in Sweden. In contrast to these studies on Nordic countries, Oreopoulos et al. (2006), relying on variation in the school minimum age across states and time in the US, demonstrate that increasing the education of either parent has a negative and significant effect on the probability that a child repeats a year of school. This decline in grade repetition by children as a consequence of an increase in parental schooling is also found in France (Maurin and McNally 2008). Using changes in the mandatory schooling laws implemented in Britain during the seventies, Chevalier (2004) finds evidence of large, positive effects of maternal education on her child's education but no significant effects of fathers' education.

Taken together, these IV studies do not present a clear picture and reveal that, while there is a large set of estimates of intergenerational mobility from a wide range of different countries, the literature to date has not included a comparative analysis of educational reforms undertaken at the country level. This observation strengthens my claim that using this variation in European compulsory schooling laws is a novel contribution to the literature that can improve our understanding of how and why parental education affects children's outcomes by accounting for institutional and cultural factors across different European countries.

1.3 Data

The data used in this study are drawn from the first two waves of the Survey of Health, Ageing and Retirement in Europe (SHARE), which took place in 2004 and 2006 in nine different European countries.[2] This survey interviews individuals aged fifty and above who speak the official language of each country, and do not live abroad or in an institution, plus their spouses or partners irrespective of age. The main advantage of this data source is the representativeness of the sample of elderly people in Europe because this survey is constructed to ensure comparability of the analysis across the different countries. Furthermore, this survey is harmonized with the U.S. Health and Retirement Study (HRS) and the English Longitudinal Study of Ageing (ELSA). The survey also contains detailed informa-

[2] Altogether, 15 countries are covered by the first and the second waves, but I consider only a sub-group of 9 countries for which I have information on educational reforms during the period between 1920 and 1956. Following Brunello et al. (2012), I exclude Spain and Greece because the compulsory schooling laws occurred too late to identify a treatment group.

tion on a broad set of variables: demographics, socio-economic characteristics, self-reported health as well as social and family networks. In this paper, I present evidence for nine countries, for which I could compute some key educational variables. These countries cover the various regions of continental Europe, ranging from Scandinavia (Sweden and Denmark) through Central Europe (Austria, Belgium, France, Germany and the Netherlands), and from the Mediterranean area (Italy) to Eastern Europe (Czech Republic).

I also employ data on reforms in the minimum school leaving age across the above-mentioned European countries, relying on recent works by Brunello et al. (2009), Brunello et al. (2012) and Garrouste (2010). As in Brunello et al. (2012), Table 1 presents a historical overview of the educational reforms that affected cohorts of parents from the 1930s until the late 1960s: for each country, the table reports the year of the reform,[3] the *pivotal cohort* (i.e., the year of birth of the first cohort affected by the reform), the change in the minimum school leaving age and in the years of compulsory schooling prescribed by the law, and the age at school entry. It is worth noticing that the countries selected for this study have extended the school leaving age by one year or more, and that the Netherlands and the Czech Republic experienced only a temporary reduction in the years of compulsory schooling.[4] Strikingly, although Italy had a lower initial level of mandatory schooling (5 years), it made substantial improvements during the postwar period (8 years). Note also that, as the schooling reforms in the West German states occurred at different points in time, Table 1 presents information on these reforms at the state level.[5]

[3]The listed year corresponds to the year when a certain reform was passed, which may not be equal to year of implementation (e.g., the Austrian reform of 1962 was implemented in 1966; the French reform of 1959 was implemented in 1967).

[4]More details on the educational reforms in the Netherlands can be found in van Kippersluis et al. (2011) and Brunello et al. (2012).

[5]See Pischke and von Wachter (2008) for more information on the educational reforms in the West German states.

Table 1: Compulsory School Reforms, by Country

Country	Reform year	Pivotal cohort	Change in min. school leaving age	Years of comp. educ.	Age at school entry
Austria	1962/66	1951	14 to 15	8 to 9	6
Belgium (Flanders)	1953	1939	14 to 15	8 to 9	6
Czech Republic	1948	1934	14 to 15	8 to 9	6
	1953	1939	15 to 14	9 to 8	6
	1960	1947	14 to 15	8 to 9	6
Denmark	1958	1947	11 to 14	4 to 7	7
France	1936	1923	13 to 14	7 to 8	6
	1959/67	1953	14 to 16	8 to 10	6
Germany (Baden-Wuerttemberg)	1967	1953	14 to 15	8 to 9	6
Germany (Bayern)	1969	1955	14 to 15	8 to 9	6
Germany (Bremen)	1958	1943	14 to 15	8 to 9	6
Germany (Hamburg)	1949	1934	14 to 15	8 to 9	6
Germany (Hessen)	1967	1953	14 to 15	8 to 9	6
Germany (Niedersachsen)	1962	1947	14 to 15	8 to 9	6
Germany (Nordrhein-Westfalen)	1967	1953	14 to 15	8 to 9	6
Germany (Rheinland-Pfalz)	1967	1953	14 to 15	8 to 9	6
Germany (Saarland)	1964	1949	14 to 15	8 to 9	6
Germany (Schleswig-Holstein)	1956	1941	14 to 15	8 to 9	6
Italy	1963	1949	11 to 14	5 to 8	6
Netherlands	1942	1929	13 to 14	7 to 8	6
	1947	1933	14 to 13	8 to 7	6
	1950	1936	13 to 15	7 to 9	6
Sweden	1949	1936	13 to 14	6 to 7	7
	1962	1950	14 to 16	7 to 9	7

Notes: Source: Brunello et al. (2012).

The key variables of interest in this analysis are the educational attainment of parents and children. I measure educational attainment using years of schooling. One unusual feature of the dataset I employ is that it contains direct information on years of schooling for both parents and children. However, while for countries in the first wave the data on years of education are provided and are defined according to the ISCED-97 criteria,[6] for countries in the second wave there is information available on the country specific ISCED-97 codes but not on years of education. In my analysis, the Czech Republic is the only country included in the second wave that is not present in the first wave. I addressed this lack of information on the Czech Republic by taking advantage of the country specific conversion table that allowed me to recode the ISCED-97 codes into years of schooling.[7] It is also important to note that the measurement error due to misreporting could be magnified by the fact that children's educational achievement is reported by their parents.

To construct the sample of parents, I restrict attention to married or cohabiting individuals with at least one biological child, and, following Brunello et al. (2012), I focus on the cohorts of parents born from 1920 through 1956.[8] These cohorts were affected by the reforms of mandatory schooling that gradually came into effect across the European countries. By comparing the year of birth with the pivotal cohort, I am able to determine whether parents were exposed to the changes in schooling laws. For the analysis of this paper, it is worth stressing that I focus only on mothers and fathers who are the *family respondents*, i.e., the first member of the couple interviewed, who was entitled to respond to questions in the children's section on behalf of the couple. This implies that, while information on parental education was reported directly by both spouses, the data

[6]See http://www.unesco.org/education/information/nfsunesco/doc/isced_1997.htm for details on ISCED coding.

[7]The conversion table for the Czech Republic, which is not present in the Release Guide 2.5.0 for waves 1 and 2, was provided by the SHARE Country Team for the Czech Republic.

[8]After 1956, there is a substantial drop in the number of family respondents. The reason is that SHARE interviews people who are 50+. Therefore, for the 2006 wave, the people targeted by SHARE were born in 1956 or before.

on the children's characteristics, such as years of schooling, were collected from the family respondents.[9] Therefore, parents who are not the family respondents are not considered in my sample of parents. I then link the demographic and educational characteristics of each child to the data for the corresponding family respondent to create an intergenerational dataset. Because the early cohorts of parents are likely to be affected by the consequences of World War II that might have forced them to interrupt or delay their academic careers, in the robustness analysis I also construct a postwar sample that includes the birth cohorts of parents born between 1935 and 1956, and show that the results are robust to excluding the prewar cohorts.

In this paper, I restrict attention to first born children.[10] The cohorts of interest were born between 1956 and 1980**.** This interval presents two advantages: first, it guarantees the absence of an overlap between parents and their offspring that could potentially undermine the exclusion restriction of the instrument; second, it allows me to consider sufficiently old children who were at least 24 years old at the time of the interview.[11] The distributions of the samples of parents and children across the countries are presented in Table 2.

[9]The family respondents answer the questions of the children's section. The couple's first person interviewed is the family respondent in the coverscreen. Because the family respondents are selected exclusively on the basis of the chronological order of interviews per couple, the sample of parents can be arguably considered as a random sample. For further details, please see the Release Guide 2.5.0 for waves 1 and 2.

[10]In SHARE, questions about children's education are asked for a maximum of four children. Table A3 in Appendix A displays the cross-country distribution of first-born and later-born children and Table A4 reports the descriptive statistics. Importantly, in Table A5 and A6 I show that the main results remain substantially unchanged when including all children, both first-born and later-born children, thereby making my results relevant beyond the first-born children.

[11]The first SHARE interview took place in 2004 for all countries with the exception of the Czech Republic, which was surveyed in 2006. Table A7 in Appendix A reports the descriptive statistics for children born after 1980 that are excluded from the analysis. As expected, the sample size is greatly reduced because there is a small fraction of parents that had their children after 1980.

Table 2: Sample of Parents and Children, by Country

Sample	**Parents (1920-1956)**			**Children (1956-1980)**		
	Fathers	Mothers	Total	Sons	Daughters	Total
Austria	312	170	482	225	257	482
Belgium	454	275	729	363	366	729
Czech Republic	360	363	723	363	360	723
Denmark	174	146	320	155	165	320
France	372	268	640	332	308	640
Germany	339	333	672	346	326	672
Italy	464	487	951	495	456	951
Netherlands	464	460	924	465	459	924
Sweden	369	374	743	373	370	743
Total	3,308	2,876	6,184	3,117	3,067	6,184

Notes: All the samples contain individuals for whom information on education is not missing.

After these restrictions, the final full sample of parents consists of 6,184 family respondents: 3,308 (53.5%) fathers and 2,876 (46.5%) mothers, while the final sample of children consists of 6,184 siblings: 3,117 (50.4%) sons and 3,067 (49.6%) daughters.[12] The summary statistics reported in Table 3 indicate, as expected, that fathers are older and are slightly more educated than their spouses. Particularly striking is that the second generation of children has a considerably higher level of schooling than their parents (13.25 versus 10.71 years of schooling). However, part of the positive association between parents' and children's education might reflect the positive correlation with unobserved ability.

[12] All of these samples contain individuals for whom information on educational attainment is not missing. Table A1 in Appendix A reports the number of observations that are lost due to missing data on parents' and children's years of schooling for each country. Overall, it is reassuring to notice that the total number of missing values is relatively very low (87 individuals). Table A2 reports the distribution of the postwar sample of parents across the countries.

Table 3: Summary Statistics, Sample of Parents (1920-1956) and Children (1956-1980)

Variable	Observations	Mean	Std. Dev.
Children			
Age	6,184	35.89	6.50
Education	6,184	13.25	2.84
Female (%)	6,184	0.49	0.5
Mothers and Fathers together			
Age	6,184	61.85	7.19
Education	6,184	10.71	3.68
Household size	6,184	2.4	0.79
Fathers			
Age	3,308	63.03	7.39
Education	3,308	10.98	3.74
Mothers			
Age	2,876	60.50	6.69
Education	2,876	10.40	3.59

Notes: All the samples include individuals for whom information on education is not missing. Education is measured with years of schooling and is defined according to the ISCED-97 criteria.

In Figure 1, I analyze differences in the pattern of educational attainment between the cohorts of parents and children across countries. To facilitate comparisons, I separate the countries into two groups: in one group, the Northern European countries (Sweden, Denmark and the Netherlands, see Figure 1a); and in the other, the Western (Austria, Germany, Belgium and France), Southern (Italy) and Eastern (Czech Republic) European countries (see Figure 1b).[13] The vertical and horizontal axes describe the average number of years of schooling and year of birth, respectively. The vertical dashed line marks the year 1956 to separate the two samples. As one could expect, in all countries there is a clear trend of rising levels of education, so that one might be concerned that it may be difficult to distinguish the effect of the reform from the secular trend. Ideally, to thoroughly address this issue, one would like to rely on a very large sample of parents born in the close vicinity of the schooling law. Unfortunately, the sample size of my dataset is too small to conduct this local analysis.

[13] Because my sample includes only one Southern European country (Italy) and only one Eastern European country (Czech Republic), I included these two countries in a separate row with the Western European countries.

Figure 1: Trend in Education of Parents and Children

Figure 1a: Northern Europe

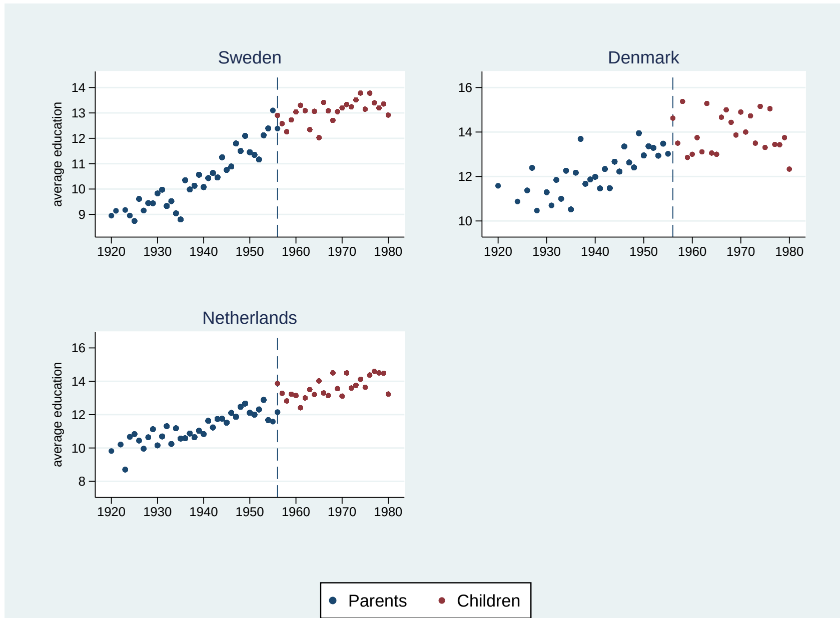
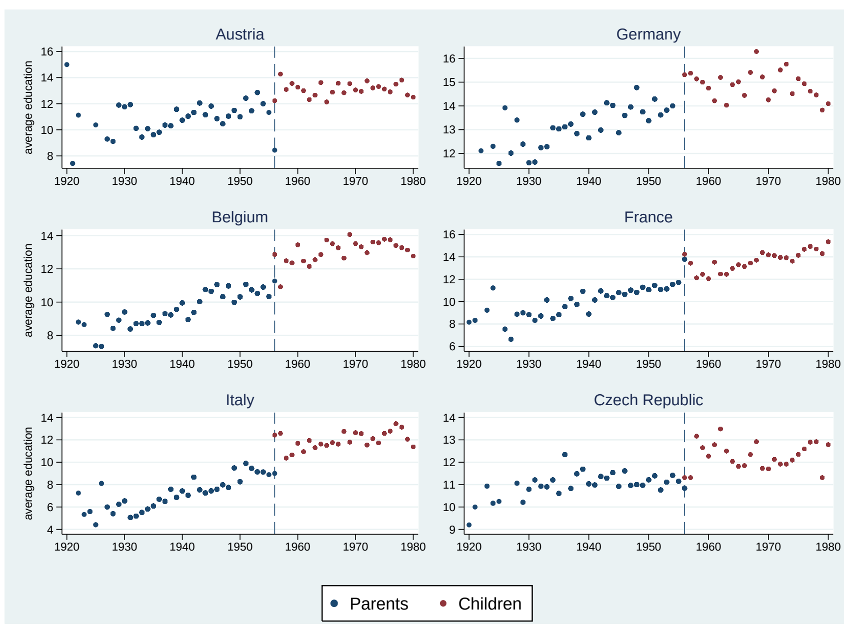


Figure 1b: Western, Southern and Eastern Europe



Notes: The vertical and horizontal axes describe the average number of years of schooling and year of birth, respectively. The vertical dashed line marks the year 1956 to distinguish between the sample of parents (1920-1956) and children (1956-1980). Because in my sample there is only one Southern European country (Italy) and only one Eastern European country (Czech Republic), I put these two countries in a separate row together with the Western European countries.

1.4 Empirical Specification

Following Black et al. (2005) and Oreopoulos et al. (2006), I specify a model for the children's education in a multi-country framework as follows:

$$Edu^c_{ihj} = \alpha + \beta Edu^p_{ihj} + \gamma X_{ihj} + \tau^p + \tau^c + \eta_j + \epsilon^p_{ihj} \tag{1.1}$$

where the unit of observation i denotes the child-parent pair and the superscripts c and p refer to child and parental characteristics, respectively. The dependent variable Edu^c_{ihj} denotes the years of schooling of the offspring generation, observed for child i within household h residing in country j and is expressed as a linear function of parental education levels measured by the years of schooling of the family respondent Edu^p_{ihj}. A key element of my approach is the inclusion of both country fixed effects η_j that account for time-invariant, unobserved characteristics, such as institutional and cultural features, that are likely to vary by country, and birth cohort fixed effects for parents τ^p (in 1-year intervals), which capture any systematic differences in school outcomes across parental birth cohorts. In model (1.1), I then include birth cohort fixed effects for children τ^c (in 1-year intervals) to control for cohort trends in education and account for the possibility that some children might not have finished school at the time of the interview.[14] In some specifications, I also control for country-specific quadratic trends in parental birth cohorts because the implementation of the schooling reforms might be correlated with country-level, unobserved, time-varying factors. Because many of the socio-economic characteristics of the parents tend to be endogenous, as they are themselves affected by the parent's education, I use a parsimonious specification: I add a set of individual socio-demographic characteristics X_{ihj}, including the children's gender and household size. Finally, ϵ_{ihj} represents an idiosyncratic error

[14]One might argue that the birth year of the child is a potentially endogenous variable because parents can choose the timing of birth. However, in the robustness checks I show that the main results hold even when excluding cohort fixed effects for children.

term. It is reasonable to believe that ϵ_{ihj} is correlated with the outcome variable because it embodies the unobserved factors of parents, including ability, which might affect the academic performance of the children.

To distinguish between the intergenerational effects of mothers and fathers, in model (1.1) I also include the interaction between parental education and the gender dummy for the parents. By including this interaction, I am able to capture the differential impacts of maternal and paternal education on children's education. Formally, I estimate the following specification:

$$Edu^c_{ihj} = \alpha + \beta Edu^p_{ihj} + \lambda Edu^p_{ihj} * gender^p_{ihj} + \gamma X_{ihj} + \tau^p + \tau^c + \eta_j + \epsilon^p_{ihj} \tag{1.2}$$

where $gender^p_{ihj}$ is equal to one if the family respondent is the mother.

1.4.1 Identification strategy

I identify the causal effect of parental education on children's education using compulsory schooling laws over 30 years as an instrument for the number of years of parental schooling. A large body of economic literature (among others, Black et al. 2005; Oreopoulos et al. 2006) recognizes this identification strategy as valid because changes in compulsory schooling laws produce variation in parental education that is credibly exogenous and unlikely to be related to unobservable characteristics of the parents, such as ability, that might explain the different educational outcomes of their offspring.

In this study, I apply this IV strategy to a European framework by instrumenting parental education with the number of years of compulsory schooling determined by the law.[15] This multi-country approach has been employed by Brunello et al. (2009) to study

[15]The fact that the identification of the effects of the reforms is made possible through differences in the timing of the changes in these laws across countries suggests some similarities with a differences-in-differences design.

the returns to schooling and Brunello et al. (2011) to investigate the effects of schooling on health. Formally, the instrument is constructed as follows:

$$Reform^p_{ij} = \begin{cases} (ycs)^A & if \;\; (parental\ year\ of\ birth)_i > (pivotal\ cohort)_j \\ \\ (ycs)^B & otherwise \end{cases} \tag{1.3}$$

where the variable ycs represents the number of years of compulsory schooling, and the superscripts B and A denote before and after the educational reform, respectively. Therefore, I construct the instrument in such a way that it depends on three factors: the country j in which the reform took place, the parents' years of birth, and the first birth cohort affected by the reform (i.e., the pivotal cohort). I can then determine whether parents were exposed to the compulsory laws by comparing their years of birth with those of the pivotal cohort.

Model (1.1) is estimated using two stage least squares (2SLS), and the first stage regression is given by:

$$Edu^p_{ihj} = \delta_0 + \delta_1 Reform^p_{ij} + \pi X_{ihj} + \varphi^p + \varphi^c + \sigma_j + \upsilon_{ihj} \tag{1.4}$$

where Edu^p_{ihj} is instrumented with $Reform^p_{ij}$, the compulsory years of schooling in the respective country and cohort. Similarly, the first stage for model (1.2) can be written as:

$$Edu^p_{ihj} = \delta_0 + \delta_1 Reform^p_{ij} + \delta_2 Reform^p_{ij} * gender^p_{ihj} + \pi X_{ihj} + \varphi^p + \varphi^c + \sigma_j + \upsilon_{ihj} \tag{1.5}$$

Therefore, in equation (1.5) I employ not only the years of compulsory schooling but also the interaction between compulsory schooling and the gender of the parent as instruments. There are two points to note on this instrumental variables strategy. First, because it varies over parental cohorts and across countries, the instrument is affected by two potential sources of serial correlation: within country over parental cohorts and across countries for the same parental cohort. To mitigate this concern, I cluster all standard errors by the country and cohort of the parents, thus allowing for arbitrary dependence within country-cohort cells.[16] Second, the compulsory schooling reforms do not affect the entire population. Rather, these reforms influence only the least educated groups of parents. As a consequence, this identification strategy allows me to recover a Local Average Treatment Effect (LATE) instead of averages across the population (ATE). As noted by Card (2001), these local effects are of interest because the groups of individuals captured by the LATE are those that are most likely to be affected by the mandatory schooling laws.

1.5 Main Results

1.5.1 Association between the Schooling of Parents and their Children

Table 4 presents the results from a simple ordinary least squares (OLS) estimation of model (1.1). In column 1, I report the coefficient of parental education without other controls: the OLS estimate suggests that a one year increase in the parents' years of schooling is associated with an increase in the number of years of schooling for children of 0.32 years. This coefficient is significant and robust to the inclusion of controls for parental birth cohort and socio-demographic characteristics (column 2) including the gender of the children and household size. When controlling separately for country fixed effects (column 3) and cohort fixed effects for children (column 4), parental education remains positively and significantly associated with children's education, although the coefficients are slightly reduced to 0.30 and 0.29, respectively. I then include a full set of country indicators interacted with a quadratic trend in the parents' year of birth (column 5). The results are virtually unchanged relative to the previous specification.

[16] As for Germany, given that the instrument varies at the state level, clustering occurs at the level of the West German states. However, to account for potential correlation across West German states, I also cluster at the level of Germany finding that the 2SLS standard errors are virtually identical.

Table 4: Effects of Parents' Education, Naive OLS

Dependent Variable: Child's Education	(1)	(2)	(3)	(4)	(5)
Parental education	0.325***	0.327***	0.305***	0.286***	0.286***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Female (child)		0.204***	0.217***	0.225***	0.225***
		(0.066)	(0.065)	(0.063)	(0.064)
Household size		-0.135***	-0.121***	-0.170***	-0.176***
		(0.042)	(0.043)	(0.043)	(0.043)
Socio-demographic controls	NO	YES	YES	YES	YES
Cohort F.E. for parents	NO	YES	YES	YES	YES
Country F.E.	NO	NO	YES	YES	YES
Cohort F.E. for children	NO	NO	NO	YES	YES
Country-specific quadratic trends	NO	NO	NO	NO	YES
Observations	6,184	6,184	6,184	6,184	6,184
R^2	0.178	0.190	0.228	0.243	0.247
Mean of Dep. Var.	13.25				
Std. Dev. of Dep. Var.	2.84				

Notes: Birth cohort dummies for parents and children are in 1-year intervals. Country-specific quadratic cohort trends are computed by interacting parental birth cohort and its square with country dummies. Standard errors clustered at the parents' country and cohort level are reported in parentheses.

* Significant at 10%; ** significant at 5%; *** significant at 1%.

To allow for separate effects of maternal and paternal education, I estimate model (1.2), in which I include the interaction between parental education and a female dummy that takes value one if the family respondent is the mother. The estimates for the most general specification are reported in Table 5. Column 1 corresponds to column 4 of Table 4. The inclusion of the interaction term (see column 2) reduces the magnitude of the coefficient on parental education, but the OLS estimate remains positive and significant. While I find only a slightly stronger relationship between maternal education and children's outcomes than between the children's and paternal education, the coefficient on the interaction term is highly statistically significant. Interestingly, this positive sign is consistent with the view (see, for example, Black et al. 2005; Chevalier 2004; Chevalier et al. 2011) that mothers are likely to devote more time to child care than fathers. This finding is discussed later in the paper. Finally, in Table 5, I present similar results from dividing the sample into sons (column 3) and daughters (column 4).

Table 5: Effects of Parents' Education on Sons and Daughters, Naive OLS

Dependent Variable: Child's Education	(1)	(2)	(3)	(4)
Sample	Overall	Overall	Sons	Daughters
Parental education	0.286***	0.231***	0.230***	0.228***
	(0.011)	(0.013)	(0.020)	(0.017)
Parental educ*female (parent)		0.041***	0.044***	0.040***
		(0.006)	(0.009)	(0.008)
Household size	-0.176***	-0.172***	-0.154**	-0.176***
	(0.043)	(0.043)	(0.063)	(0.055)
Observations	6,184	6,184	3,117	3,067
Mean of Dep. Var.	13.25	13.25	13.14	13.34
Std. Dev. of Dep. Var.	2.84	2.84	2.89	2.78

Notes: All specifications include controls for country dummies, birth cohort dummies for parents and children (in 1-year intervals) and country-specific quadratic cohort trends (computed by interacting parental birth cohort and its square with country dummies). Standard errors clustered at the parents' country and cohort level are reported in parentheses.

* Significant at 10%; ** significant at 5%; *** significant at 1%.

Overall, my OLS estimates confirm a strong, positive intergenerational correlation in education even when country fixed effects are controlled for and the sample is divided into sons and daughters. However, this positive correlation could be explained by the role family background characteristics played in determining the children's level of educational attainment, or it might reflect genetic differences in ability that are transmitted to the children. In the next subsection, I attempt to establish whether this positive correlation has a causal interpretation.

Furthermore, it is not surprising that in all specifications I do find a negative and statistically significant correlation between family size and children's schooling performance. In the more comprehensive specification (see column 5 in Table 4), a one unit increase in the household size is associated with a 0.18 years decline in child education. This result appears to be in line with the notion that there might be a trade-off between child quantity and quality (Becker and Lewis 1973).

1.5.2 Causality between Schooling of the Parents and their Children

In Panel A of Tables 6, 9 and 10, I present the two stage least squares (2SLS) estimates, which are the primary estimates of interest in this study. To instrument for parental education, I use years of compulsory schooling. Table 6 (Panel A) indicates that, in the first two specifications, the coefficient on parental education is strongly statistically significant (at the 1 percent level); adding country fixed effects (column 3) and cohort fixed effects for children (column 4) to the model reduces the significance of the 2SLS estimate, but it is maintained at the 10 percent threshold. The magnitude of the effect of parental education varies remarkably with the specification and becomes substantially larger when country fixed effects are added to the model (see column 3).

As emphasized by Holmlund et al. (2011), for the validity of the instrument to hold, it is important to control not only for country fixed effects but also for country-specific time trends to disentangle the identifying variation in parental education induced by the compulsory schooling reforms from the confounding factors that arise from country-level, upward trends in educational attainment. When I add country-specific quadratic trends in birth cohorts to the model (column 5), I find that the estimated effects of parental education are no longer statistically significant. While this lack of significance raises concerns about the ability of my analysis to properly distinguish between the compulsory schooling effects and the positive trends in average educational attainment of the parents, I argue below that this result can likely be explained by the fact that the inclusion of country-specific trends markedly reduces the first stage power of my instrument. Interestingly, this weak first stage relationship between the instrument and parental education when including country-specific trends has also been found in Oreopoulos et al. (2006), who point to the presence of contemporaneous trends of increasing both average educational attainment of the parents and years of compulsory schooling.

Table 6: Effects of Parents' Education, IV analysis

Panel A: 2SLS					
Dependent Variable: Child's Education	(1)	(2)	(3)	(4)	(5)
Parental education	0.281***	0.367***	0.498**	0.437*	0.468
	(0.057)	(0.054)	(0.254)	(0.262)	(0.334)
Female (child)		0.206***	0.224***	0.229***	0.238***
		(0.066)	(0.066)	(0.065)	(0.069)
Household size		-0.114**	-0.123***	-0.150***	-0.159**
		(0.048)	(0.044)	(0.056)	(0.069)
Observations	6,184	6,184	6,184	6,184	6,184
R^2	0.175	0.188	0.179	0.214	0.202
Mean of Dep. Var.	13.25				
Std. Dev. of Dep. Var.	2.84				
First stage F statistic	42.99	38.23	8.58	7.47	1.63
Panel B: First stage					
Dependent Variable: Parent's Education	(1)	(2)	(3)	(4)	(5)
Compulsory education	0.632***	0.604***	0.217***	0.207***	0.104
	(0.096)	(0.098)	(0.074)	(0.076)	(0.081)
Observations	6,184	6,184	6,184	6,184	6,184
R^2	0.062	0.081	0.221	0.258	0.262

Notes: Birth cohort dummies for parents and children are in 1-year intervals. Country-specific quadratic cohort trends are computed by interacting parental birth cohort and its square with country dummies. Standard errors clustered at the parents' country and cohort level are reported in parentheses.

* Significant at 10%; ** significant at 5%; *** significant at 1%.

Thus, I investigate the first stage estimates reported in Panel B of Table 6. These estimates show that the reform is strongly and positively correlated with the number of years of parental schooling and that its t statistic is above 2.7 even when conditioning on country and cohort fixed effects. One notable exception, however, is the model that includes the country-specific trends (column 5), in which the first stage estimate is not statistically different from zero, with the t statistic of approximately 1.3. Panel A of Table 6 also reports the corresponding first stage F-test statistic for each specification that accounts for the clustering of the standard errors at the parents' country and cohort level. When subsequently including country fixed effects and cohort fixed effects for children (columns 3 and 4), this statistic falls to approximately 7.5, which is below the cutoff value of 10 suggested by Bound et al. (1995) and Staiger and Stock (1997), but this value is substantially higher than the first stage F-test statistic produced by the model that incorporates the country-specific trends (column 5). Because of the lack of power in my identification strategy after controlling for country-specific trends, I choose the specification that does not allow for country-specific trends (see column 4 of Table 6, Panel A) as my preferred one. In this model, my results reveal that parental education appears to have a large causal effect on children's education: I find that an additional year of parental education will raise a child's educational attainment by 0.44 of a year.

I also perform a number of weak-instrument robust tests that allow me to conduct inference that has the correct size even in the presence of weak instruments. The results of this set of tests are presented in Table 7, which provides the Anderson-Rubin (AR) statistic (Anderson and Rubin 1949) and, as a reference, the standard Wald test for specifications 3 and 4 in Panel A.[17] As one could expect given the relatively low value of the first stage

[17] Because my model is just-identified, the conditional likelihood-ratio (CLR) test converges to the AR test, so there is no need to report both. In cases where the IV model contains more than one instrumental variable, additional weak-instrument robust tests, such as the LM test, are presented. Notice that these tests can only be applied to a model with one endogenous variable. A discussion of these issues can be found in Finlay et al. (2009).

regression F-test statistic, the AR p-value and confidence intervals are larger than the non-robust Wald counterparts, but the differences are limited. Most importantly, the AR p-value is still on the border of statistical significance at approximately the 10 percent threshold. These results imply that, even when accounting for the presence of a weak instrument, the treatment effects of parental education remain marginally statistically significant.

Table 7: Weak-Instrument Robust Tests for models (3) and (4) in Table 6 - Panel A

Endogenous Variable: Parents' Education	(3)		(4)	
	p-value	95% C. Set	p-value	95% C. Set
Anderson-Rubin	0.069	[-0.054, 1.292]	0.134	[-0.216, 1.235]
Wald	0.049	[0, 0.996]	0.095	[-0.076, 0.951]

Notes: Wald test is not robust to weak instruments.

For the above reasons, and in light of the Angrist and Pischke (2009) claim that "just-identified 2SLS is approximately unbiased", I conclude that in my preferred specification (see Table 6, Panel A, column 4) the issue of weak instrument bias may be of less concern, and that there is some evidence of a causal effect of parental education on the educational attainment of their children. Table 8 summarizes the results of my favorite model. The first column reports the OLS estimates from a regression of the child's education on the education of the parents. In the second column, I display the reduced form coefficient from a regression of the child's education on the instrument. In the third column, I present the first stage estimate from a regression of parents' education on the instrument. In the last column, I present the 2SLS estimate, where years of compulsory schooling are used as an instrument for parents' education. This latter estimate is simply the reduced form estimate divided by the first stage estimate.

Table 8: Effects of parental education in the preferred model

	(1)	(2)	(3)	(4)
	OLS	Reduced-Form	First stage	IV
Dependent Variable:	Child's Education	Child's Education	Parental Education	Child's Education
parental education	0.286***			0.437*
	(0.011)			(0.262)
compulsory education		0.091	0.207***	
		(0.061)	(0.076)	
Observations	6,184	6,184	6,184	6,184
R^2	0.243	0.141	0.258	0.214
First stage F statistic	7.47			
Anderson-Rubin test p-value	0.134			

Notes: All specifications include controls for country dummies, birth cohort dummies for parents and children (in 1-year intervals), and socio-demographic characteristics. Standard errors clustered at the parents' country and cohort level are reported in parentheses.

* Significant at 10%; ** significant at 5%; *** significant at 1%.

While the main goal of this study is the analysis of the effect of parental education on the second generation's education, another contribution is the exploration of the different roles fathers and mothers play in explaining the transmission of human capital to their sons and daughters. To conduct this analysis, I proceed in two steps.

First, by adding an interaction between the gender of the parent and parental education to the model (see model (1.2)), I am able to partially extend the analysis by allowing for different effects of maternal and paternal education. This means that my preferred model (see Table 6, Panel A, column 4) uses as instruments not only the years of compulsory schooling but also interaction term between compulsory schooling and the gender of the parent. The 2SLS estimates, reported in Panel A of Table 9, suggest that when controlling for the differential impacts of mothers and fathers (see column 2), the results remain substantially unchanged with respect to the direction, magnitude and significance. Consistent with the results of the OLS estimates, I find that the coefficient on the interaction between years of education and parental gender is highly statistically significant (at the 1% level) and positive, thus suggesting that maternal education is somewhat more important than paternal.

Table 9: Effects of Parents' Education on Sons and Daughters, IV w/o country-specific trends

	(1)	(2)	(3)	(4)
Sample	Overall	Overall	Sons	Daughters
Panel A: 2SLS				
Dep. Var.: Child's Education				
Parental education	0.437*	0.462*	0.553*	0.410
	(0.262)	(0.269)	(0.300)	(0.573)
Parental educ*female (parent)		0.050***	0.058***	0.044*
		(0.013)	(0.017)	(0.023)
Observations	6,184	6,184	3,117	3,067
Mean of Dep. Var.	13.25	13.25	13.14	13.34
Std. Dev. of Dep. Var.	2.84	2.84	2.89	2.78
Angrist-Pischke first stage F statistic	7.47	6.99	7.56	2.00
Panel B: First stage				
Dep. Var.: Parent's Education				
Compulsory education	0.207***	0.226***	0.301***	0.153
	(0.076)	(0.077)	(0.102)	(0.094)
Compulsory educ*female (parent)		-0.052***	-0.055***	-0.048***
		(0.012)	(0.016)	(0.017)
Observations	6,184	6,184	3,117	3,067

Notes: All specifications include controls for country dummies, birth cohort dummies for parents and children (in 1-year intervals) and socio-demographic characteristics.

* Significant at 10%; ** significant at 5%; *** significant at 1%.

Second, in an attempt to disentangle the treatment effects of parental schooling on sons from the effects on daughters, I separately consider samples of male and female children. The results for sons and daughters are presented in columns 3 and 4 (Table 9, Panel A), respectively. When conducting the analysis for sons, the coefficient on parental education is statistically significant and larger than the coefficient generated by the full sample (0.55 versus 0.46 years), although the effect is less precisely estimated given the smaller sample size. On the contrary, when examining the sample of daughters, the 2SLS estimate for parental education falls to approximately 0.41 and is not statistically different from zero. In columns 3 and 4, I also find evidence that maternal education seems to matter more than paternal education in determining the educational success of their offspring. I explain these findings in the discussion of the results.

The non-significant effects of parental education on daughters can be largely attributed to the weak first stage relationship between the reform and number of years of parental schooling (see Table 9, Panel B, column 4): the t statistic for the reform is approximately 1.6 for daughters compared to approximately 3 for sons (column 3). Furthermore, the Angrist-Pischke first stage F-test statistic is approximately 2 for daughters compared to approximately 7.6 for sons. The first stage estimates also reveal that the reform had a stronger impact on fathers.

In Table 10, I repeat the analysis in Table 9 using the country-specific quadratic trends: the coefficient on parental education is statistically significant, although very noisy, only for the sample of daughters. As expected, in the first stage regression (see Table 10, Panel B) the reform shows no evidence of being correlated with parental schooling. Interestingly, the coefficient on the interaction term between the gender of the parent and parental education remains statistically different from zero across all specifications, thus supporting the basic finding that mothers have a significant stronger effect than fathers on the academic outcomes of their offspring.

Table 10: Effects of Parents' Education on Sons and Daughters, IV with country-specific trends

	(1)	(2)	(3)	(4)
Sample	Overall	Overall	Sons	Daughters
Panel A: 2SLS				
Dep. Var.: Child's Education				
Parental education	0.468	0.421	1.064	0.671**
	(0.334)	(0.272)	(0.973)	(0.334)
Parental educ*female (parent)		0.047***	0.079*	0.052***
		(0.013)	(0.043)	(0.015)
Observations	6,184	6,184	3,117	3,067
Mean of Dep. Var.	13.25	13.25	13.14	13.34
Std. Dev. of Dep. Var.	2.84	2.84	2.89	2.78
Angrist-Pischke first stage F statistic	1.63	8.64	1.39	4.04
Panel B: First stage				
Dep. Var.: Parent's Education				
Compulsory education	0.104	0.124	0.153	0.092
	(0.081)	(0.082)	(0.109)	(0.108)
Compulsory educ*female (parent)		-0.051***	-0.055***	-0.045***
		(0.012)	(0.016)	(0.017)
Observations	6,184	6,184	3,117	3,067

Notes: All specifications include controls for country dummies, birth cohort dummies for parents and children (in 1-year intervals) and socio-demographic characteristics.

* Significant at 10%; ** significant at 5%; *** significant at 1%.

Regardless of the model, I find that the IV estimates are larger than their OLS counterparts. While this result might appear to contradict intuition regarding omitted variable bias given the positive correlation between parental education and unobserved ability, it is consistent with several studies that employ mandatory schooling reforms as instrument. Part of this difference can be attributed to two explanations (Card 2001). First, because there might be important measurement errors in the self-reported schooling of the parents, the resulting downward bias could be significantly larger than the upward omitted variable bias. Second, as mentioned previously, this IV strategy captures the effect of reforms only on the part of the population that is induced to obtain additional schooling by the educational reforms. Therefore, the treatment effect of parental education for this subset of compliers is likely to be above the average marginal effect for the entire population.[18] The ratios of the IV estimates to the OLS estimates for the entire sample and sub-samples of sons and daughters range from 1.5 to 2.4. Similar ratios have been found in Oreopoulos et al. (2006), Angrist and Krueger (1991) and Staiger and Stock (1997).

[18] An additional explanation is that there might be some correlation between the instrument and the unobserved factors that affect a child's outcome. However, previous studies using this variation have not questioned the exclusion restriction of the instrument.

1.6 Robustness Checks

In this section, I perform a variety of robustness checks to test how the results change when I modify the sample or use a different instrument (see Tables 11 and 12).

Table 11: Robustness Checks, 2SLS estimates

Dep. Var.: Child's Education	(1)	(2)	(3)	(4)
Sample	Overall	Overall	Sons	Daughters
Panel A: Post-WWII sample of parents (1935-1956)				
Parental education	0.470*	0.496*	0.558*	0.513
	(0.259)	(0.266)	(0.293)	(0.599)
Parental educ*female (parent)		0.049***	0.056***	0.046***
		(0.010)	(0.014)	(0.017)
Observations	5,247	5,247	2,639	2,608
Panel B: w/o cohort F.E. for children				
Parental education	0.498**	0.518*	0.603**	0.405
	(0.254)	(0.266)	(0.279)	(0.642)
Parental educ*female (parent)		0.042**	0.057***	0.026
		(0.020)	(0.022)	(0.047)
Observations	6,184	6,184	3,117	3,067

Table 11 (cont.ed): Robustness Checks, 2SLS estimates

Dep. Var.: Child's Education	(1)	(2)	(3)	(4)
Sample	Overall	Overall	Sons	Daughters
Panel C: Parent's years of schooling<11				
Parental education	0.980**	0.982**	0.614	1.479
	(0.492)	(0.495)	(0.603)	(1.112)
Parental educ*female (parent)		0.048***	0.057***	0.048*
		(0.015)	(0.020)	(0.026)
Observations	2,829	2,829	1,407	1,422
Panel D: Narrow windows around the pivotal cohorts (+/- 6 years)				
Parental education	0.813***	0.658***	0.896***	0.651*
	(0.147)	(0.188)	(0.138)	(0.391)
Parental educ*female (parent)		0.049***	0.054***	0.052***
		(0.010)	(0.012)	(0.015)
Observations	2,804	2,804	1,382	1,422
Panel E: Binary instrument				
Parental education	0.334**	0.360**	0.418**	0.302
	(0.153)	(0.146)	(0.190)	(0.242)
Parental educ*female (parent)		0.039***	0.043***	0.036***
		(0.008)	(0.012)	(0.011)
Observations	6,184	6,184	3,117	3,067

Table 12: Robustness Checks, first stage estimates

Dep. Var.: Parent's Education	(1)	(2)	(3)	(4)
Sample	Overall	Overall	Sons	Daughters
Panel A: Post-WWII sample of parents (1935-1956)				
Compulsory education	0.228***	0.241***	0.321***	0.151
	(0.082)	(0.083)	(0.104)	(0.102)
Compulsory educ*female (parent)		-0.034***	-0.039**	-0.029
		(0.013)	(0.017)	(0.018)
Observations	5,247	5,247	2,639	2,608
Panel B: w/o cohort F.E. for children				
Compulsory education	0.217***	0.226***	0.301***	0.153
	(0.074)	(0.077)	(0.102)	(0.094)
Compulsory educ*female (parent)		-0.053***	-0.055***	-0.048***
		(0.012)	(0.016)	(0.017)
Observations	6,184	6,184	3,117	3,067
Panel C: Parent's years of schooling<11				
Compulsory education	0.145***	0.148***	0.166**	0.110
	(0.047)	(0.047)	(0.071)	(0.079)
Compulsory educ*female (parent)		-0.007	-0.004	-0.008
		(0.008)	(0.011)	(0.011)
Observations	2,829	2,829	1,407	1,422

Table 12 (cont.ed): Robustness Checks, first stage estimates

Dep. Var.: Parent's Education	(1)	(2)	(3)	(4)
Sample	Overall	Overall	Sons	Daughters
Panel D: Narrow windows around the pivotal cohorts (+/- 6 years)				
Compulsory education	0.222**	0.236**	0.306**	0.173
	(0.094)	(0.096)	(0.124)	(0.129)
Compulsory educ*female (parent)		-0.034**	-0.040*	-0.028
		(0.016)	(0.022)	(0.025)
Observations	2,804	2,804	1,382	1,422
Panel E: Binary instrument				
First reform	0.429***	0.477***	0.701***	0.242
	(0.160)	(0.169)	(0.225)	(0.231)
Second reform	0.620***	0.799***	0.853**	0.710**
	(0.204)	(0.228)	(0.343)	(0.307)
First reform*female (parent)		-0.107	-0.178	-0.030
		(0.129)	(0.163)	(0.174)
Second reform*female (parent)		-0.473*	-0.476	-0.407
		(0.273)	(0.453)	(0.564)
Observations	6,184	6,184	3,117	3,067

I begin by investigating whether my estimates are sensitive to WWII. The major concern here is that, despite the inclusion of cohort fixed effects, the older cohorts of parents tend to be positively selected on their health and other unobservable factors because these individuals are still alive and able to participate in the SHARE surveys. While SHARE data do not allow for the elimination of survivor bias and the identification of a sample entirely unaffected by WWII, I can construct a postwar sample that takes into account the consequences of WWII that might have influenced the educational decisions of the early cohorts of parents by leading them to interrupt or postpone their academic careers. This postwar sample contains the younger cohorts of parents born during the 1935-1956 period. The 2SLS estimates reported in Panel A show that the effect of parental education is slightly larger once the prewar cohorts are dropped, but this model displays an identical pattern relative to the baseline specification (see Table 9, Panel A): the estimate increases from 0.49 to 0.55 years once I move from the full sample to the sample of sons and then decreases to 0.51 years and becomes insignificant when I consider the sample of daughters. The more pronounced impact of maternal education on children's schooling persists, consistent with the baseline specification. Therefore, the results are quite robust to excluding the prewar cohorts.

I also investigate the robustness of my results to the exclusion of the child's year of birth. There might be a concern that the year of the child's birth is an endogenous decision because it may be affected by the level of parental education. In Panel B, I show that the coefficients of interest are very similar to the main specification with regard to the direction, magnitude and significance, with the only difference being that the mother's schooling no longer has an impact on daughters.

As a third check, following Black et al. (2005) and Oreopoulos et al. (2006), I conduct the analysis on the sample of the less educated parents who are most likely to be affected by reforms in mandatory schooling. Therefore, I examine the subset of children whose parents have 11 or fewer years of education. The 2SLS estimates presented in Panel C are similar in direction and significance to the benchmark specification, but the results are much less statistically precise. The lack of precision of the estimates is largely due to the small sample size. Contrary to my expectation, I find the estimated coefficients to be much larger in magnitude: the reduced number of observations is likely to bias my results, thus limiting this type of analysis. The first stage estimates (see Table 12, Panel C) indicate, as expected, that compulsory schooling laws are strongly correlated with lower levels of parental schooling, except for daughters.

While the small sample size severely limits the possibility of using cross-country school reforms as a regression discontinuity, one can imagine taking a narrow window of parental birth cohorts around the pivotal cohorts to correct for the impact of any long-run trends across birth cohorts. To do this, I restrict the sample to children with their parents born six years before or after the change in the laws.[19] The results reported in Panel D are consistent with my baseline model: I find evidence of a causal impact, although larger in magnitude, of parental education and a larger impact of maternal education on children's schooling.

As a final check, I assess the sensitivity of my estimates to the use of an alternative definition of the instrument. I construct a binary reform variable which is set to one for a given country for the post-reforms cohorts of parents, i.e., if parental year of birth exceeds the pivotal cohort. This allows me to distinguish between the treated and untreated cohorts of parents. Formally:

$$Treat^{p}_{ij} = \begin{cases} 1 & if \ \ (parental\ year\ of\ birth)_i > (pivotal\ cohort)_j \\ \\ 0 & otherwise \end{cases} \tag{1.6}$$

where the variable $Treat^{p}_{ij}$ is now an indicator that takes value 1 if parent i who resides in country j belongs to a birth cohort that was exposed to the schooling reform. This implies that the treated individuals are born after the pivotal cohort. Importantly, some

[19] For the countries with more than one reform, I consider only the most recent reform.

countries implemented more than one compulsory schooling law during my observation period: two laws were implemented in Sweden and France and three laws were implemented in the Netherlands and the Czech Republic. For the group of countries with more than one reform, I construct a treatment dummy for each additional reform using the same procedure as defined in (1.6).[20] Therefore, the number of indicators corresponds to the number of within country reforms. For the analysis in this study, it is important to note that the indicators are set to zero when additional reforms did not take place in a given country. One weakness of this binary instrument compared to the previous instrument based on the years of compulsory schooling is that it does not adequately capture the magnitude of the reform: a reform raising the number of years of compulsory schooling by one year (Austria, for example) is treated in the same manner as a reform increasing compulsory schooling by more than one year (Italy, for example). In this setup, the first stage is given by:

$$Edu^{p}_{ihj} = \delta_0 + \delta_1 Treat^{p}_{ij,l} + \pi X_{ihj} + \varphi^{p} + \varphi^{c} + \sigma_j + \upsilon_{ihj} \ , \ l = 1, 2 \tag{1.7}$$

as mentioned above, $Treat^{p}_{ij,l}$ is a binary variable that equals 1 if the parent i in country j was affected by the $l-th$ educational reform and 0 otherwise.

The results are presented in Panel E. As expected, the magnitude of the effect of parental education is smaller than in the benchmark specification (see Table 9, Panel A), but, most importantly, the estimated coefficients remain unchanged with respect to the direction and significance in the full sample as well as in the sub-samples of sons and daughters.

[20] The third reform will not be used for causal interpretation because its identification would come only from the Czech Republic and the Netherlands.

1.7 Discussion

In this study, I found that maternal education is more important than paternal education for the academic achievement of children. While this finding is consistent with the established IV literature on the intergenerational transmission of human capital (Black et al. 2005; Chevalier 2004; Chevalier et al. 2011), the mechanisms through which a mother's education may affect her child's education are not entirely clear. In their studies, Chevalier (2004) and Chevalier et al. (2011) emphasize that the stronger effects of maternal education can be largely explained by the role of the mother as the main provider of childcare within the family. For example, mothers tend to spend more time breastfeeding, reading to their children, helping them with homework, and taking them outside. As noted by Black et al. (2003), this stronger effect of maternal education could also be attributed to other mechanisms such as positive assortative mating or the quantity/quality trade-off. However, because educated mothers are more likely to work, they may also have less time to stay at home and less time to devote to child care. However, Carneiro et al. (2013) counter that more educated mothers do not spend less time with their children partly because they have fewer children or less leisure time. They conclude that increased employment among more educated mothers does not have negative effects on children.

Whether it is plausible to assume that the intergenerational mobility coefficient is the same across different countries remains unexplored. To account for cross-country heterogeneity in parents' education, I add to model (1) a full set of interactions between parental years of schooling and the country dummies instrumented by the corresponding interactions between years of compulsory schooling and the country indicators. I then test the joint significance of this array of country-specific slopes in parental years of education and demonstrate that I do not reject the null hypothesis that the treatment effect of parental schooling is the same for all countries.[21] An alternative strategy to allow for the maximum level of heterogeneity at the country level would be to estimate separate models for each

[21] The results are available from the author upon request.

country. However, the number of observations in each country is too small to identify the treatment effects of parental education.[22] Overall, the evidence presented above suggests that the IV strategy on the pooled sample with common coefficients on all the variables is most appropriate for the data used in the present investigation.

1.8 Conclusion

An important component of human capital can be assessed by the extent of individuals' academic careers, measured by the number of years of education. When considering policies that improve the educational outcomes of new generations, a key question concerns the causal role of parents' education in influencing the educational outcomes of their offspring, that is, the intergenerational transmission of human capital. Does the increased education of parents cause higher levels of education of their children? Or, is the observed correlation between parents' and children's levels of education naive and due to unobserved covariates, such as innate ability? Are there differences in the education effects of mothers versus fathers, on daughters versus sons?

Although a large literature has attempted to answer these questions, the evidence remains largely mixed. In this paper, I employ the changes in compulsory schooling laws in Europe over the period 1920-1956 to explore the effect of parental education on the schooling performance of their children. In my preferred model, I do find some evidence of a causal relationship between parental and children's education. Specifically, I find that an additional year of parental education induced by the reform generates 0.44 years of additional schooling for their children. Furthermore, I provide evidence that the mother's schooling has a stronger impact than her husband's in determining the educational success of their offspring. This latter result is robust to the inclusion of the country-specific trends.

The findings of this paper reveal that increasing the education of less educated parents

[22]The results are available from the author upon request. This country-specific analysis is usually not conducted in the economic literature using SHARE data (see, for example, Alessie et al. 2013; Brunello et al. 2009; Brunello et al. 2011; Brunello et al. 2012). Furthermore, the inclusion of only one country in the Mediterranean area (Italy) and one in Eastern Europe (Czech Republic) makes it difficult to produce separate estimates by European regions.

might have beneficial effects not only on the targetd generation but also on the educational outcomes of the next generation because family background characteristics affect the process of intergenerational transmission of human capital. These results highlight the long-term effectiveness of compulsory schooling laws in improving intergenerational outcomes in education. A mother's stronger influence over children's education suggests that supporting the education of mothers may represent an important avenue for educational policies.

References

Alessie R, Angelini V, van Santen P (2013) Pension wealth and household savings in Europe: Evidence from SHARELIFE. European Economic Review, forthcoming.

Anderson TW, Rubin H (1949) Estimation of the parameters of a single equation in a complete system of stochastics equations. Annals of Mathematical Statistics 20(1): 46-63

Angrist JD, Krueger AB (1991) Does compulsory school attendance affect schooling and earnings? Quarterly Journal of Economics 106: 979-1014

Angrist JD, Pischke JS (2009) Mostly Harmless Econometrics: An Empiricist's Companion. Princeton University Press, Princeton

Antonovics KL, Goldberger AS (2005) Does Increasing Women's Schooling Raise the Schooling of the Next Generation? Comment. American Economic Review 95(5): 1738-1744

Becker G, Lewis HG (1973) On the Interaction Between the Quantity and Quality of Children. Journal of Political Economy 81(2): 279-288

Behrman JR, Rosenzweig MR (2002) Does Increasing Women's Schooling Raise the Schooling of the Next Generation? American Economic Review 92(1): 323-334

Björklund A, Lindahl M, Plug E (2006) The Origins of Intergenerational Associations: Lessons from Swedish Adoption Data. Quarterly Journal of Economics 121(3): 999-1028

Björklund A, Salvanes KG (2010) Education and Family Background: Mechanisms and Policies. NHH Dept. of Economics Discussion Paper, n. 14

Black SE, Devereux PJ, Salvanes KG (2005) Why the apple doesn't fall far: understanding intergenerational transmission of human capital. American Economic Review 95(1): 437-449

Black SE, Devereux PJ, Salvanes KG (2003) Why the apple doesn't fall far: understanding intergenerational transmission of human capital. NBER Working Paper, n. 10

Black SE, Devereux PJ (2010) Recent developments in intergenerational mobility. In: Handbook of Labor Economics, Elsevier, Amsterdam

Bound J, Jaeger DA, Baker R (1995) Problems with instrumental variables estimation when the correlation between the instruments and the endogenous explanatory variables is weak. Journal of the American Statistical Association 90(430): 443-450

Brunello G, Fort M, Weber G (2009) Changes in Compulsory Schooling, Education and the Distribution of Wages in Europe. Economic Journal 119: 516-539

Brunello G, Fort M, Schneeweis N, Winter-Ebmer R (2011) The Causal Effect of Health on Education: What is the Role of Health Behaviors? IZA Discussion Paper, n. 5944

Brunello G, Weber G, Weiss CT (2012) Books are forever: Early life conditions, education and lifetime income. IZA Discussion Paper, n. 6386

Card D (2001) Estimating the Return to Schooling: Progress on some Persistent Econometric Problems. Econometrica, 69(5): 1127-1160

Carneiro P, Meghir C, Parey M (2013) Maternal Education, Home Environments, and the Development of Children and Adolescents. Journal of the European Economic Association 11(1): 123-160

Chevalier A, Harmon C, O'Sullivan V, Walker I (2011) The Impact of Parental Earnings and Education on the Schooling of Children. Working Paper, Geary Institute, University College Dublin

Chevalier, A (2004) Parental Education and Child's Education: A Natural Experiment. IZA Discussion paper n. 1153

Finlay K, Magnusson LM (2009) Implementing Weak Instrument Robust Tests for a General Class of Instrumental Variables Models. Stata Journal 9(3): 1-26

Garrouste C (2010) 100 Years of educational reforms in Europe: A contextual database. In: EUR - Scientific and Technical Research series, Publications Office of the European Union, Luxembourg

Holmlund H, Lindhal M, Plug E (2011) The causal effect of parents' schooling on children's schooling: A comparison of estimation methods. Journal of Economic Literature 49(3): 615-651

Imbens GW, Angrist JD (1994) Identification and Estimation of Local Average Treatment Effects. Econometrica 62(2): 467-475

Maurin E, McNally S (2008) Vive la Révolution! Long Term Returns of 1968 to the Angry Students. Journal of Labor Economics 26(1): 1-33

Oreopoulos P, Page ME, Stevens AH (2006) The Intergenerational Effects of compulsory Schooling. Journal of Labor Economics 24(4): 729-760

Pischke JS, von Wachter T (2008) Zero Returns to Compulsory Schooling in Germany: Evidence and Interpretation. Review of Economics and Statistics 90(3): 592-598

Plug E (2004) Estimating the Effect of Mother's Schooling on Children's Schooling Using a Sample of Adoptees. American Economic Review 94(1): 358-368

Sacerdote B (2002) The Nature and Nurture of Economic Outcomes. American Economic Review 92(2): 344-348

SHARE Release Guide 2.5.0 for waves 1 & 2 (2011) http://www.share-project.org/sharelife/

Staiger D, Stock JH (1997) Instrumental variables regression with weak instruments. Econometrica 65(3): 557-586

van Kippersluis H, O'Donnell O, van Doorslaer E (2011) Long Run Returns to Education: Does Schooling Lead to an Extended Old Age? Journal of Human Resources 46(4): 695-721

Appendix A: Supplemental Tables

Table A1: Missing data on education, by Country

Sample	Parents (1920-1956)	Children (1956-1980)	Total
Austria	1	3	4
Belgium	3	7	10
Czech Republic	0	4	4
Denmark	1	3	4
France	8	8	12
Germany	6	3	9
Italy	0	4	4
Netherlands	14	13	25
Sweden	9	7	15
Total	42	52	87

Notes: The total amount takes into account the fact that there are seven cases of overlapping missing values between the sample of parents and children (four observations in France, two in the Netherlands and one in Sweden).

Table A2: Post-WWII Sample of Parents (1935-1956), by Country

Country	Fathers	Mothers	Total
Austria	261	150	411
Belgium	358	240	598
Czech Republic	295	351	646
Denmark	148	131	279
France	299	241	540
Germany	283	300	583
Italy	356	438	794
Netherlands	367	409	776
Sweden	292	328	620
Total	2,659	2,588	5,247

Notes: All the samples contain individuals for whom information on education is not missing.

Table A3: Sample of first-born and later-born children (1956-1980), by Country

	First-born			**Second-born**			**Third-born**			**Fourth-born**			**Total**		
	Sons	Dau.	Tot	Sons	Dau.	Tot	Sons	Dau.	Tot	Sons	Dau.	Tot	Sons	Dau.	Tot
AT	225	257	482	172	171	343	67	63	130	24	21	45	488	512	1000
BE	363	366	729	281	259	540	120	100	220	52	37	89	816	762	1578
CZ	363	360	723	292	283	575	75	64	139	15	11	26	745	718	1463
DK	155	165	320	149	135	284	49	52	101	17	8	25	370	360	730
FR	332	308	640	224	247	471	102	96	198	33	29	62	691	680	1371
DE	346	326	672	252	218	470	90	80	170	20	20	40	708	644	1352
IT	495	456	951	369	339	708	141	125	266	45	43	88	1050	963	2013
NL	465	459	924	393	373	766	159	152	311	58	62	120	1075	1046	2121
SE	373	370	743	318	318	636	108	114	222	32	32	64	831	834	1665
Tot	3117	3067	6184	2450	2343	4793	911	846	1757	296	263	559	6774	6519	13293

Notes: All samples contain individuals for whom information on education is not missing. In SHARE, questions on the children's education are asked to a maximum of four children.

Table A4: Summary Statistics, first-born and later-born children (1956-1980)

Variable	Observations	Mean	Std. Dev.
First-born children			
Education	6,184	13.25	2.84
Female (%)	6,184	0.49	0.5
Second-born children			
Education	4,793	13.07	2.82
Female (%)	4,793	0.49	0.49
Third-born children			
Education	1,757	12.76	2.90
Female (%)	1,757	0.48	0.49
Fourth-born children			
Education	559	12.43	2.98
Female (%)	559	0.47	0.49

Notes: All the samples include individuals for whom information on education is not missing. Education is measured with years of schooling and is defined according to the ISCED-97 criteria.

Table A5: First-born and later-born children pooled together, IV w/o country-specific trends

	(1)	(2)	(3)	(4)
Sample	Overall	Overall	Sons	Daughters
Panel A: 2SLS				
Dep. Var.: Child's Education				
Parental education	0.495**	0.538**	0.824***	-0.062
	(0.239)	(0.266)	(0.305)	(0.503)
Parental educ*female (parent)		0.054***	0.075***	0.017
		(0.018)	(0.023)	(0.031)
Observations	13,293	13,293	6,774	6,519
Mean of Dep. Var.	13.08	13.08	13	13.16
Std. Dev. of Dep. Var.	2.85	2.85	2.91	2.79
Angrist-Pischke first stage F statistic	6.93	6.11	7.64	2.03
Panel B: First stage				
Dep. Var.: Parent's Education				
Compulsory education	0.203***	0.229***	0.291***	0.167*
	(0.077)	(0.078)	(0.093)	(0.093)
Compulsory educ*female (parent)		-0.082***	-0.085***	-0.077***
		(0.013)	(0.014)	(0.016)
Observations	13,293	13,293	6,774	6,519

Notes: All specifications include controls for country dummies, birth cohort dummies for parents and children (in 1-year intervals) and socio-demographic characteristics.

* Significant at 10%; ** significant at 5%; *** significant at 1%.

Table A6: First-born and later-born children pooled together, IV with country-specific trends

	(1)	(2)	(3)	(4)
Sample	Overall	Overall	Sons	Daughters
Panel A: 2SLS				
Dep. Var.: Child's Education				
Parental education	0.782	0.965	1.278*	0.776
	(0.616)	(0.865)	(0.745)	(1.286)
Parental educ*female (parent)		0.080	0.105**	0.072
		(0.055)	(0.051)	(0.075)
Observations	13,293	13,293	6,774	6,519
Mean of Dep. Var.	13.08	13.08	13	13.16
Std. Dev. of Dep. Var.	2.85	2.85	2.91	2.79
Angrist-Pischke first stage F statistic	1.65	1.20	2.66	0.82
Panel B: First stage				
Dep. Var.: Parent's Education				
Compulsory education	0.103	0.131	0.202**	0.065
	(0.081)	(0.082)	(0.099)	(0.101)
Compulsory educ*female (parent)		-0.081***	-0.085***	-0.075***
		(0.013)	(0.014)	(0.016)
Observations	13,293	13,293	6,774	6,519

Notes: All specifications include controls for country dummies, birth cohort dummies for parents and children (in 1-year intervals), socio-demographic characteristics and country-specific quadratic cohort trends (computed by interacting parental birth cohort and its square with country dummies). The Angrist-Pischke first stage F

Table A7: Summary Statistics, Sample of Parents (1920-1956) and Children (>1980)

Variable	Observations	Mean	Std. Dev.
First-born children			
Age	545	21.18	2.18
Education	545	12.15	2.28
Female (%)	545	0.49	0.05
Mothers and Fathers together			
Age	545	53.59	3.73
Education	545	12.29	3.56
Household size	545	3.46	1.01
Fathers			
Age	353	53.88	4.06
Education	353	12.30	3.56
Mothers			
Age	192	53.04	2.95
Education	192	12.28	3.57

Notes: All the samples include individuals for whom information on education is not missing. Education is measured with years of schooling and is defined according to the ISCED-97 criteria.

Table A8: First-born children born after 1980 - OLS, 2SLS and first stage w/o country-specific trends

	(1)	(2)	(3)	(4)
Sample	Overall	Overall	Sons	Daughters
Panel A: OLS - Children born after 1980				
Dep. Var.: Child's Education				
Parental education	0.145***	0.159***	0.180***	0.126**
	(0.031)	(0.035)	(0.053)	(0.061)
Parental educ*female (parent)		-0.011	-0.005	-0.014
		(0.014)	(0.020)	(0.020)
Observations	545	545	279	266
Mean of Dep. Var.	12.15	12.15	11.95	12.36
Std. Dev. of Dep. Var.	2.28	2.28	2.26	2.28
Panel B: 2SLS - Children born after 1980				
Dep. Var.: Child's Education				
Parental education	-0.025	-0.007	-0.033	0.178
	(0.305)	(0.305)	(0.234)	(0.389)
Parental educ*female (parent)		-0.013	-0.014	-0.014
		(0.015)	(0.019)	(0.025)
Observations	545	545	279	266
Mean of Dep. Var.	12.15	12.15	11.95	12.36
Std. Dev. of Dep. Var.	2.28	2.28	2.26	2.28
Angrist-Pischke first stage F statistic	5.34	5.30	5.73	1.31

Table A8 (cont.ed): First-born children born after 1980 - OLS, 2SLS and first stage w/o country-specific trends

	(1)	(2)	(3)	(4)
Sample	Overall	Overall	Sons	Daughters
Panel C: First stage - Children born after 1980				
Dep. Var.: Parent's Education				
Compulsory education	0.544**	0.562**	0.914**	0.450
	(0.236)	(0.244)	(0.380)	(0.406)
Compulsory educ*female (parent)		-0.024	-0.052	0.030
		(0.039)	(0.062)	(0.055)
Observations	545	545	279	266

Notes: All specifications include controls for country dummies, birth cohort dummies for parents and children (in 1-year intervals) and socio-demographic characteristics. The Angrist-Pischke first stage F statistic refers to the first stage regression of parental education; the first stage regression of parental education*female has much stronger power, thus the Angrist-Pischke first stage F statistic is omitted. Standard errors clustered at the parents' country and cohort level are reported in parentheses.

* Significant at 10%; ** significant at 5%; *** significant at 1%.

Chapter 2

Living Arrangements in Europe: Whether and Why Paternal Retirement Matters

Abstract: This paper uses retrospective micro data from eleven European countries to investigate the role of paternal retirement in explaining children's decisions to leave the parental home. To assess causality, I use a bivariate discrete hazard model with shared frailty and exploit over time and cross-country variation in early retirement legislation. Overall, the results indicate a positive and significant influence of paternal retirement on the probability of first nest-leaving of children residing in Southern European countries, both for sons and daughters. By contrast, there is no evidence of significant effects on children living in Northern and Central European countries. I then discuss and test empirically the potential mechanisms by which paternal retirement may affect children's nest-leaving. My results suggest that the increase in children's nest-leaving around paternal retirement does not appear to be justified by changes in parents' budget constraints or in the supply of informal child care provided by grandparents. Rather, one must probably look for channels involving negative externalities in preferences between parents and children.

2.1 Introduction

Over the last few years, a substantial body of research has attempted to identify some of the potential determinants that may induce youths to continue living with their parents. While this investigation is particularly relevant for Italy and some other southern European countries, such as Spain and Greece, where young people tend to remain with their parents until their late 20s and early 30s, leaving home only when they get married, the way children respond to these factors has attracted increasing attention in the public policy debate of most European countries. For example, policymakers may be interested in reducing the adverse impact of delayed cohabitation on an array of children's outcomes, including individual motivations and ambitions, reservation wages, labor market entry and geographical mobility (Billari and Tabellini 2010). A further cause of concern regards the phenomenon of falling fertility rates associated with prolonged coresidence. Combined with the effects of population aging, this phenomenon raises the elderly dependency ratio, thereby contributing to placing extra pressure on the long-term financial sustainability of pension systems.

There is consensus in recent literature that in Italy parental retirement induces a significant decline in the number of grown children living with their parents; however, researchers are still puzzled about the possible mechanisms underlying this relationship. There are two major competing explanations for this pattern. On the one hand, Manacorda and Moretti (2006) argue that retired parents are no longer able to make a financial transfer to their children and thus are unable to bribe them to stay at home because of the drop in their post-retirement income. On the other hand, Battistin et al. (2009) emphasize that liquidity considerations are unlikely to play a role because most Italian employees receive a generous lump-sum payment upon retirement. Therefore, they suggest that parents may use part of their severance payment to help their children leave the nest, which may account for most of the decline in consumption around the time of retirement. While these two studies differ in many respects, they have two important common traits. First, they use Italy as a case study. The Italian case is of particular interest because Italy is among the European countries with the highest age for home-leaving and because it is one of the very few European countries in which workers are entitled to receive a large severance payment at the time of retirement. A second similarity is that both studies employ an instrumental variable (IV) approach that obtains identification from Italian pension reforms that substantially changed the eligibility conditions for retirement during the 1990s.

Overall, the lack of a cross-country analysis severely limits the ability to clarify whether the housing emancipation of young adults upon parents' retirement can be attributed to liquidity problems faced by parents, as suggested by Manacorda and Moretti (2006), or to the receipt of a sizeable retirement allowance, as noted by Battistin et al. (2009). Thus, there is the need for empirical work to test which of the channels dominates in practice. I contribute to previous studies by taking advantage of a European dataset to test and discuss the relative weight of these two competing hypotheses and shed some light into the mechanism. To address problems of reverse causation, I estimate a bivariate discrete hazard model with shared frailty (Abbring and van den Berg 2003; 2005) for the impact of paternal retirement on the timing of children's nest-leaving. Furthermore, to provide random variation in the timing of paternal retirement, I strengthen my identification strategy by employing changes in eligibility rules for early retirement benefits that were implemented across European countries and during the period 1961 to 2007 as an exclusion restriction. To the best of my knowledge, this is the first paper that makes use of this exogenous source of variation to children's living arrangements to assess whether and to what extent paternal retirement caused their children to leave the nest. Compared to the linear IV strategy, the hazard specification provides a more appropriate statistical framework for modeling time-to-event/survival outcomes and accounting for *right-censoring*, thereby allowing me to overcome certain limitations faced by previous IV studies. The bivariate hazard model finally offers greater flexibility in handling nonlinear baseline hazards and nonlinear effects of covariates and provides a novel approach to identifying treatment effects by modeling unobserved heterogeneity explicitly through bivariate specification.

To conduct this analysis, I use data from the second wave (2006) of the Survey of Health, Ageing and Retirement in Europe (SHARE). This European dataset has three important features: first, it collects data on current economic, health, and family conditions of over 30,000 individuals aged fifty and above in several European countries; second, it provides retrospective information on the retirement age of the respondents and the nest-leaving ages of their children; and lastly, because it is designed to be cross-nationally comparable, this dataset enables me to properly conduct a multi-country analysis. Furthermore, I employ data about the European early retirement legislation by relying on Angelini et al. (2009), Mazzonna et al. (2012) and the country-specific studies discussed in Gruber and Wise (2004). It should be stressed, however, that across the countries considered in the present investigation there are very different cultural histories, labor market institutions and social characteristics. Such differences may play a lasting role in explaining the substantial heterogeneity in the ages of children when they leave home across Europe (see, for example, Aassve et al. 2002; Billari et al. 2001) and may not be entirely captured by including country fixed effects in the model estimated on the pooled sample from multiple countries. To mitigate this concern, I conduct the main analysis by European region. These regions correspond to the geographical aggregation into northern European countries (Sweden, Denmark and the Netherlands), central European countries (Austria, Germany, Switzerland, France and Belgium) and southern European countries (Italy, Spain and Greece). This aggregation is particularly relevant because it reflects profound differences in welfare states and family regimes across the above-mentioned country groups (see, for example, Albertini et al. 2007, 2012). One implication of this division is that the conditional impact of early retirement eligibility rules on paternal retirement and children's nest-leaving outcomes is allowed to vary between northern, central and southern European countries.

Based on these data, my main results demonstrate the following: a) Paternal retirement has a positive and significant effect on the timing of children's nest-leaving in southern European countries. In this European region, the magnitude of the effect varies between 1.4% and 5.5%, and there are no significant differences between sons and daughters. b) The mechanism through which this pattern may occur remains an open issue because it cannot be attributed to families' liquidity problems or a severance payment at the time of paternal retirement. One must probably look for channels involving negative externalities in preferences between parents and children. c) In northern and central Europe, there is no evidence that children's nest-leaving outcomes are significantly affected by paternal retirement. These findings are robust to a number of specification checks. On the policy side, the results of this paper suggest that in southern Europe there are potentially unintended and undesirable consequences of pension reforms on moving-out decisions of young people.

The remainder of the paper is organized as follows. The next section discusses the relevant literature on children's nest-leaving. Section 3 presents a description of the data and provides background information on eligibility ages for retirement in Europe. Section 4 describes the empirical specification and identification strategy. The main results of the paper are presented in Section 5, and Section 6 illustrates the robustness checks. I discuss the results in Section 7, and concluding remarks are provided in Section 8.

2.2 Literature Review

A vast economic literature has investigated the channels that may affect young individuals' living arrangements. Most papers have focused on parental and children's economic resources, youth labor market conditions, the prevailing characteristics in housing markets and cultural factors. Among these channels, the father's resources around the time of retirement plays a dominant role. As discussed herein, although there is consensus that parental retirement encourages the nest-leaving of Italian young adults, less is known about the mechanisms underlying their departure from the parental home. In the literature to date, there are two competing explanations for the change in the pattern of children's leaving home upon paternal retirement. The first explanation, proposed by Manacorda and Moretti (2006), concentrates on the role played by parental preferences for co-residence. Using the Italian pension reforms of the 1990s as a source of exogenous variation in household income, the authors find that the prolonged co-residence of youths can be attributed to parents' desire for cohabitation because they may be willing to give up some of their additional income due to postponed retirement to bribe their children to stay at home longer. This view would imply that once parents retire, they are no longer able to keep their children at home as a result of the decline in their post-retirement income. The second explanation, that of Battistin et al. (2009), suggests a different mechanism. According to these authors, because most Italian employees receive a sizeable severance payment upon retirement, parents may use this money to buy a house for their sons and daughters, who can then leave the parental home.[1]

These two studies, however, limit their analyses to the Italian case and do not test the implications of their findings on other European countries. Therefore, the multi-country analysis and the source of exogenous variation provided by the early retirement legislation in Europe allows this study to address questions that other researchers have not. By exploiting the intergenerational nature of the dataset, I analyze the decline in children's co-residence at the time of their fathers' retirement. In particular, I provide the first empirical test for these two competing explanations and shed some light on the specific mechanism through which this may happen. As noted by Battistin et al. (2013), there could be an additional mechanism that explains the increase in children's nest-leaving around parental retirement: for instance, if pension reforms force grandparents to stay in the labor market longer and thus reduce the time devoted to child care activities with their grandchildren. The authors find heterogeneous effects depending on the gender, with grandmothers having a significantly stronger impact on their children's fertility and nest-leaving outcomes.

This paper is also related to other contributions from the economic literature on moving-out decisions. Most notably, Becker et al. (2010) show that high rates of co-residence among young Italians can be the result of higher job insecurity compared to that of their parents, whereas Card and Lemieux (2000) find that poor labor market conditions and lower wages decrease the probability of leaving the parental nest. Another potential determinant of moving-out decisions are housing market features. Analyzing living arrangements in Italy and the Netherlands, Alessie et al. (2006) highlight that the presence of high transaction costs in housing discourages home-leaving. Finally, this paper is related to

[1] Guiso and Japelli (2002) analyze the importance of this channel, finding that economic transfers from parents contribute to earlier nest-leaving of their children.

recent literature in economics that attempts to quantify the impact of culture on economic outcomes, including children's living arrangements. The starting point of this strand of literature is the observation, by Reher (1998), that western Europe can be divided into two groups: the southern European countries, which are characterized by the existence of "strong family ties"; and the northern European countries, which are characterized by "weak family ties". According to this scholar, the late departure from the parental home is one of the indicators of "strong" family ties. Giuliano (2007) studies the impact of the sexual revolution of the 1960s on the propensity of adult children to remain in their parents' home and argues that high rates of cohabitation in southern European countries can be explained by liberalized parental attitudes towards their children's participation in pre-marital sex. She concludes that cultural traits play a major role in determining living arrangements. In a similar vein, Alesina and Giuliano (2011) provide evidence that in societies with strong family ties home production and the proportion of young adults living at home are higher, whereas labor force participation and geographical mobility are lower compared to those of societies with weak family ties.

2.3 Data and Institutional Context

In my empirical analysis, I draw data from the Survey of Health, Ageing and Retirement in Europe (SHARE). This survey collects key information on demographics, current socio-economic status, health, expectations and social and family networks for nationally representative samples of European individuals aged fifty and above who speak the official language of their respective countries, and who do not live abroad or in an institution, plus their spouses or partners irrespective of age. In this paper, I use data from the second wave collected in 2006/2007. This wave is particularly suitable for my investigation, as it provides retrospective information on the retirement years of the respondents and the year in which their children left their parental houses.[2] The main advantage of this data source concerns the representativeness of the sample of elderly individuals in Europe, because this survey is constructed to ensure the comparability of the analysis across the different countries. In this study, I present evidence from eleven countries for which I was able to collect information on the legislated early and normal ages at which individuals become eligible for a public old-age pension. These countries cover the various regions of continental Europe, ranging from Scandinavia (Sweden and Denmark), through central Europe (Austria, Belgium, France, Germany, Switzerland and the Netherlands) and the Mediterranean countries (Italy, Spain and Greece).

In my sample selection, I constrain the sample of parents to fathers because of the problems associated with labor market interruptions that typically characterize the careers of women of childbearing age. Manacorda and Moretti (2006) and Battistin et al. (2009) also focus on fathers. Moreover, I restrict my attention to fathers who were either working[3] or retired at the time of the survey, who have at least one biological child, and who were born between 1920 and 1957. Overall, these cohorts of fathers were affected by changes in the eligibility for old-age and early retirement benefits resulting from reforms that gradually came into effect across Europe over the period 1961 to 2007 to respond to the demographic transition. To construct the sample of children, I include all children, both first-born and later-born children,[4] and the cohorts of interest were born between 1940 and 1988. The choice of this interval allows me to consider virtually all the cohorts of children who were at least 18 at the time of the interview. I then link the socio-demographic characteristics of each child to the data of the corresponding father to create an intergenerational dataset.[5] After these restrictions, I obtain a working sample of parents that contains 4,935 fathers and a sample that consists of 10,720 children (5,525 sons and 5,195 daughters). The distribution of the sample of fathers as well as the sample of children across the countries

[2] Information on the year in which the respondent retired is available only for the second wave of SHARE.

[3] I use the term "working" to denote both the employed in the private or public sector and the self-employed.

[4] In SHARE, questions on the children's nest-leaving age are asked for a maximum of four children.

[5] To be more precise, I also include fathers who are not the family respondents.

is presented in Table 1.

Table 1: Sample of Fathers and Children, by Country

Sample	Fathers	Sons	Daughters	Total
Austria	242	278	255	533
Belgium	664	704	686	1,390
Denmark	407	478	421	899
France	543	588	606	1,194
Germany	568	585	546	1,131
Greece	300	339	298	637
Italy	629	655	673	1,328
Netherlands	518	593	590	1,183
Spain	361	442	385	827
Sweden	455	573	464	1,037
Switzerland	248	290	271	561
Total	4,935	5,525	5,195	10,720

Notes: This table reports the observations from the cross-sectional sample before reshaping it as a longitudinal dataset. All of the samples contain fathers for whom information on education is not missing and exclude children who were less than 18.

Descriptive statistics on the primary variables of interest are reported in Table 2. As expected, the vast majority of the fathers (72%) are retired in the interview year of wave 2, and approximately 30% of the fathers report their general health as being less than good. The individuals in my sample of children's generation are, on average, 38 years old, 52% are men and they have much better educational outcomes than their fathers (approximately

40% of adult children have completed their undergraduate or graduate studies versus 23% of the first generation).

Table 2: Summary Statistics, Sample of Fathers and Children

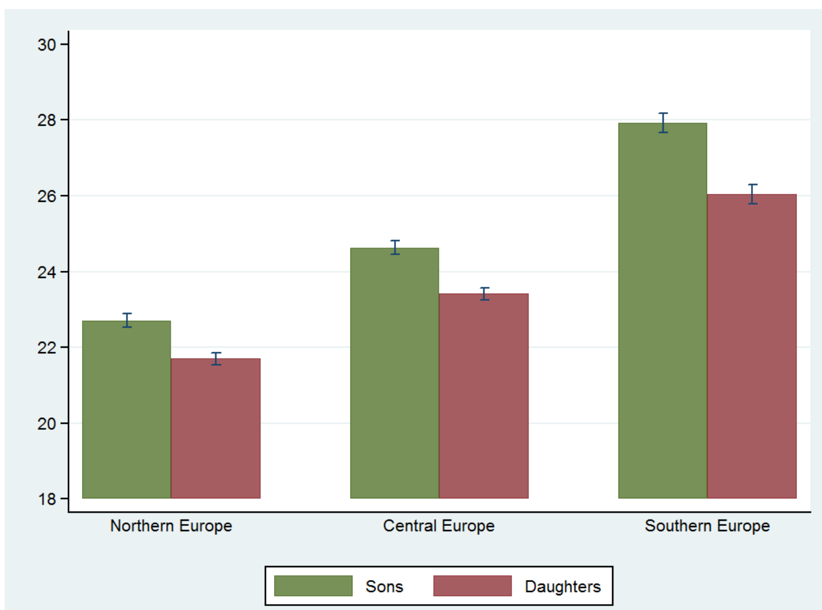
Variable	Observations	Mean	Std. Dev.
Sons			
Age	5,525	38.15	8.22
Nest-leaving age	5,525	24.92	4.83
High school	5,525	0.46	0.50
College or more	5,525	0.37	0.48
Married	5,525	0.72	0.45
Never left home	5,525	0.01	0.10
Daughters			
Age	5,195	37.77	8.42
Nest-leaving age	5,195	23.61	4.30
High school	5,195	0.46	0.50
College or more	5,195	0.40	0.49
Married	5,195	0.77	0.42
Never left home	5,195	0.01	0.10
Fathers			
Age	4,935	66.89	8.60
Retired	4,935	0.72	0.45
Working	4,935	0.28	0.45
Retirement age (retired)	3,553	60.34	4.73
High school	4,935	0.34	0.47
College or more	4,935	0.23	0.42
Bad health	4,935	0.29	0.45
Household size	4,935	2.23	0.57

To determine the retirement age of the fathers and age at which children leave the nest, I exploit recall information from the following two questions in the questionnaire asked to the parents: "In what year did you retire?" and "In what year did the child move from the parental household?". The availability of such information relating events that occurred at some point in time before the year of the survey is essential because it allows for the creation of a retrospective panel dataset. For this reason to conduct the analysis, I assume that individuals can locate past events along the time line with adequate precision. While these retrospective data are self-reported and may be susceptible to recall bias (Gibson et al. 2005), which could be amplified by the fact that children's year of home leaving is reported by their parents, the validation studies by Havari and Mazzonna (2011) and Garrouste and Paccagnella (2010) find that the fraction of memory errors is likely to be low, thereby confirming the overall accuracy of the retrospective information in the SHARE data. An important caveat of my data is worth mentioning. With the exception of the year of nest-leaving, I lack any source of time-varying information on children, such as the year of marriage, the year young people left education or their employment history. As discussed in the introduction, I conduct the main analysis by grouping countries into southern (Italy. Spain and Greece), northern (Sweden, Denmark and the Netherlands) and central (Austria, Germany, Switzerland, France and Belgium) Europe.[6] Figure 1 illustrates the mean age at which children leave the nest by gender and country group. As expected, young adults living in southern Europe moved out much later than their counterparts in the other regions. To be more specific, compared to youths in northern European countries, Italians, Spanish and Greek children left approximately five years later (26.9 years in southern Europe versus 22.1 years in northern Europe). Young people in the central European countries fall somewhere between these extremes. The figure also shows the presence of a gender gap in nest-leaving age: daughters leave the parental home

[6]Southern Europe does not include Portugal because this country, which took part in the survey from the fourth wave (2012), lacks information regarding the year in which the child left the parental home.

earlier than sons, ranging from approximately 1 year in northern and central Europe to approximately 2 years in southern Europe. This gap can partly be explained by the fact that age at marriage, which is positively correlated with the postponement of home-leaving, is lower for women. In Figure 2, I show that the proportion of married daughters is higher than that of married sons across all European regions. Interestingly, in southern Europe, the fraction of married individuals is markedly higher than that in the other regions.[7] Table 3 reports the share of adult children that left home after paternal retirement, with southern Europe showing the highest mean level, especially for sons.[8]

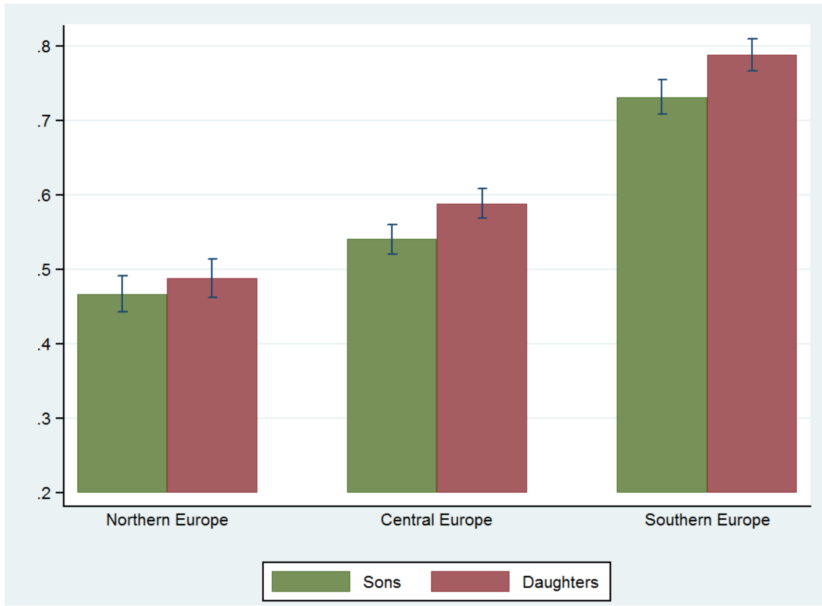
Figure 1: Children's nest-leaving mean age, by European region



[7]The dummy variable is coded as 1 for married adult children living together with the spouse during the interview year of wave 2 and 0 otherwise.

[8]Table 3 also demonstrates that gender differences within each macro-region are statistically significant.

Figure 2: Fraction of adult children who are married, by European region



Notes: Marital status refers to the interview year of wave 2. This variable is coded as 1 for married adult children living together with the spouse. Unfortunately, information on the year in which the child got married is not collected in SHARE data.

Table 3: Summary Statistics, Children who left home after paternal retirement

Sample	Sons			Daughters				Overall		
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Mean diff. *p-value*	Obs.	Mean	Std. Dev.
Southern Europe	1,436	0.45	0.49	1,356	0.38	0.48	0.00	2,792	0.42	0.49
Northern Europe	1,644	0.07	0.26	1,475	0.05	0.22	0.00	3,119	0.06	0.24
Central Europe	2,445	0.16	0.37	2,364	0.13	0.33	0.00	4,809	0.15	0.35
Overall	5,525	0.21	0.41	5,195	0.17	0.38	0.00	10.720	0.19	0.39

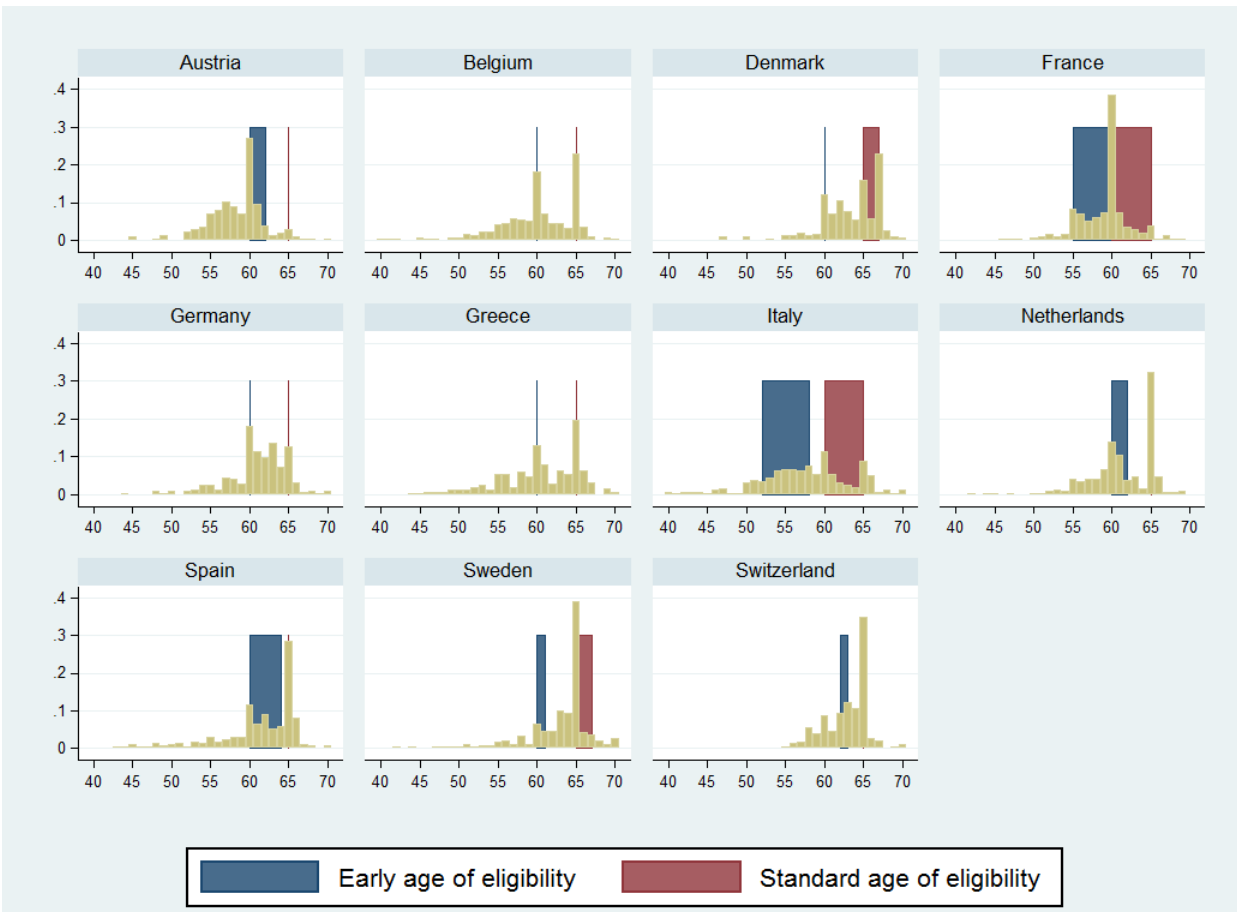
Notes: This table reports the observations from the cross-sectional sample before reshaping it as a longitudinal dataset. All of the samples contain individuals for whom information on children's nest-leaving age and paternal education is not missing and exclude children who were less than 18.

With regard to the institutional context, I use data on early eligibility ages across the above-mentioned European countries, building on the work by Angelini et al. (2009), Mazzonna et al. (2012) and Gruber and Wise (2004).[9] Figure 3 shows the distribution of the actual paternal retirement age for each country. The vertical red and blue lines denote, respectively, the eligibility ages for old-age and early retirement benefits, whereas the red and blue areas indicate changes in eligibility ages for the cohorts in my sample. As expected, there are sizeable jumps in retirement rates that occur at early and standard retirement ages. The overall picture reveals that across eleven countries with very different social security systems and labor market institutions, there are noticeable differences in many respects. For example, the normal age of eligibility for pension benefits is currently set at 65 in almost all countries, but ranges from a low of 60 in a couple of countries (Italy and France) to a high of 67 in some Nordic countries (Denmark and Sweden). A further feature worth stressing is that there is even larger multi-country variability in early

[9]Information on the retirement legislation in Greece is obtained from Duval (2003).

eligibility ages. Especially striking is that the early retirement age ranges from 52 in Italy before 1998 to 61 in Sweden after 1997.

Figure 3: Histograms of father's retirement age, by country



Notes: Source: Angelini et al. (2009), Mazzonna and Peracchi (2012), Gruber and Wise (2004) and Duval (2003). The vertical blue and red lines, respectively, mark the eligibility ages for early and normal retirement age, whereas the blue and red areas represent changes in the eligibility ages for the cohorts in my sample.

2.4 Empirical Specification

2.4.1 Bivariate Discrete-Time Hazard Model with Shared Frailty

In this section, I describe my approach to investigating the extent to which paternal retirement affects the probability of the first nest-leaving of children. To do this, I use a bivariate discrete-time hazard model with shared frailty.[10] This novel strategy to identify treatment effects in the presence of an endogenous treatment when both the treatment and outcome are survival variables was pioneered by Abbring and van den Berg (2003, 2005). This class of models is specified in terms of the hazard, defined as the conditional probability of the event occurring at a point in time provided that it has not already occurred. In this study, I am interested in jointly estimating a bivariate hazard model for the first episode of a child leaving the nest (first equation) and the first time that the father retires (second equation), allowing for correlations between the unobserved heterogeneity terms that affect these two transitions (shared frailty).[11] Formally, the model can be written in the following way:

$$\begin{cases} \theta_{1,it} & = \lambda_1(t)\,\phi_1\left(X_i\beta_1 + \delta Retired_{it} + u_{1,i}\right) \\ \\ \theta_{2,it} & = \lambda_2(t)\,\phi_2\left(X_i\beta_2 + \gamma Eligible_{it} + u_{2,i}\right) \end{cases} \tag{2.1}$$

where the unit of observation i represents the child-father pair residing in a given country, the outcome $\theta_{1,it}$ is the hazard that child i leaves the parental home at age t, $\theta_{2,it}$ refers to the hazard that father i retires at age t, and u reflects the individual-level, time-invariant, unobserved heterogeneity. The terms $\lambda_1(t)$ and $\lambda_2(t)$ represent the baseline

[10] The term frailty was first suggested by Vaupel et al. (1979) in the context of mortality studies.

[11] These two destination states are assumed to be *absorbing.* Although this assumption seems to be natural for paternal retirement, it could be somewhat less intuitive for nest-leaving because the child could go back to the parents' home after the first move-out. Because information on whether the child returned home is not available in the SHARE data, throughout the paper I assume that nest-leaving is an absorbing state.

hazard functions for the first and second equations, respectively. These functions capture the time dependence of the transitions into the two states, and they are modeled using a flexible piecewise constant function.[12] Formally, the baseline hazard can be written as follows:

$$\lambda_j(t) = \sum_{s}^{20} \lambda_{js} I_s(t) \tag{2.2}$$

where j ($j = 1, 2$) refers to the equation, s indexes the 1-year intervals, and $I_s(t)$ are dummy variables that take value 1 if the recorded duration is in the s interval. I use an open interval from $s = 19$ until the last observation leaves the sample because after 19 years the survival and censoring times occur with insufficient frequency to use finer intervals. Because I include a constant in the model, λ_{11} and λ_{21} are normalized to 0.

As for the hazard functions ϕ_1 and ϕ_2, my preferred specification uses a logistic regression. The variable X_i is a matrix of time-invariant, individual controls that may shift the hazard. Specifically, I include household size, a dummy for poor paternal health that takes value 1 if self-reported health is less than good, and an indicator for the father having a college-level education or above (ISCED$\geq$5, tertiary education) or a high school education (ISCED=3 or 4, secondary and post-secondary education). I do not include paternal occupation because of the large fraction of missing observations (approximately 30% of the cross-sectional sample); however, education is strongly correlated with occupation.[13] Both equations also entail a full set of country dummies that capture country-level, time-invariant confounding factors affecting co-residence and paternal retirement. Such factors might include, for example, cross-country cultural differences in preferences regarding co-residence and retirement, attitudes regarding partnership formation and preferences for independence. In the variable X_i, I then add birth cohort fixed effects for fathers (in 1-year intervals) to control for possible cohort trends in retirement, i.e., younger cohorts

[12] Alternatively, consistent with Melberg et al. (2010), I employ a cubic function of time, obtaining similar results.

[13] An additional issue that would arise when controlling for paternal occupation is related to how to deal with fathers who retired many years before their children's nest-leaving. Moreover, because occupation is an individual variable that usually varies over the life cycle, it is not straightforward to identify the occupational spell that really mattered for children's nest-leaving decisions.

of fathers are likely to retire later, and include controls for the birth order of the child. $Retired_{it}$ is my variable of interest and is equal to 1 if father i is retired at time t. Thus, the treatment effect δ indicates whether the child becomes more likely to leave the nest upon the father's retirement.

With regard to the unobserved heterogeneity terms $u_{1,it}$ and $u_{2,it}$, I follow the latent class approach adopted by Melberg et al. (2010) regarding the impact of cannabis on the risk of consuming hard drugs and that of Angelini et al. (2013), who evaluate the effect of illiquid assets holding on the probability of becoming a home-owner. Therefore, unobserved heterogeneity is modeled assuming a discrete distribution that has two unrestricted mass points.[14] The intuitive explanation for the presence of these two mass points is that individuals are clustered into two sub-groups that differ in terms of their unobservable propensity for nest-leaving. For instance, one group is composed of individuals who appear more likely to leave the nest later (labeled $k = 1$, Group 1, "low propensity" nest-leaving types or "late" nest-leavers), while the other is more prone to leave the parental home earlier (labeled $k = 2$, Group 2, "high propensity" nest-leaving types or "early" nest-leavers). Consistent with Melberg et al. (2010), I then allow all the coefficients to differ across the two latent groups; other studies (Pudney 2003; van Ours 2003; Salisbury 2012), in which the unobserved heterogeneity is assumed to affect only the constant term, limit this flexibility.

Allowing for correlated unobserved heterogeneity is crucial to the identification of the treatment effect δ, because there may be individual-level, unobservable factors, such as paternal ability, that determine both paternal retirement and children's decisions to leave home. If unobservable heterogeneity exists and is ignored, the estimated coefficient may be vulnerable to omitted variable bias. Moreover, the direction of the bias on the timing of nest-leaving would be unclear. For example, higher ability fathers may be more prone

[14] A discrete distribution with two mass points is a flexible parametric distribution since it does not impose any assumptions about the underlying heterogeneity other than that it can be suitably approximated by two latent classes. As noted by Melberg et al. (2010), using more than two latent classes leads to some convergence problems with the algorithm.

to retire later and may provide their children with more opportunities, thus making them more likely to leave home earlier; however, these children may also be more selective and hence more resistant to moving-out. Abbring and van den Berg (2003) show that an appealing feature of the shared frailty model is that it is identified without the need for any exclusion restrictions or assumptions about the functional form of either the baseline hazard or the joint distribution of the unobserved heterogeneity, as long as the actual timing of the treatment (paternal retirement) is random and is unaffected by the anticipation of the subsequent outcome (children's nest-leaving). However, there may still exist concerns that these two latter conditions are not entirely satisfied in model (1). The main threat to identification is that, even once correlation between frailty terms has been corrected for, the precise timing of the treatment may not occur randomly at year t, i.e., the "no anticipation" assumption is unlikely to hold. As is well known, retirement is a life event that affects various decisions of the family, including consumption, saving, fertility and labor supply.[15] For this reason, children may be able to predict when their fathers will retire, and in response to this expected event, they may modify their lifestyle behaviors and their propensity to become independent. Hence, the anticipation of paternal retirement by adult children would violate one of the key identification assumptions described above, thereby producing biased estimates. To circumvent this problem, I strengthen the identification by providing an exclusion restriction for paternal retirement. The exclusion restriction that I use is based on cross-country early retirement rules and is measured by the indicator $Eligible_{it}$, which equals 1 if father i residing in a given country was eligible for early retirement benefits at age t. These early retirement rules are not only correlated with retirement decisions (Gruber and Wise 2004), but they also provide a potentially valid instrument. Manacorda and Moretti (2006) and Battistin et al. (2009), using an IV strategy, recognize this instrument as valid because pension reforms produce variation in paternal retirement that is credibly exogenous and unlikely to be related to unobservable

[15]See, for instance, Battistin et al. (2009), Attanasio and Brugiavini (2003), Battistin et al. (2013) and Liebman et al. (2009).

characteristics of the fathers that might explain the different nest-leaving outcomes of their offspring. More importantly, it seems reasonable to argue that the timing of pension reforms came as a surprise to the fathers directly affected as well as their children. The parameters of the bivariate discrete hazard model should be interpreted in a similar fashion to a Local Average Treatment Effect (LATE) in a linear IV setting:[16] my identification captures the effect only for the subset of compliers, i.e., fathers who change their retirement decisions as a consequence of pension reforms. As a result, once the correlation between unobserved factors across both equations and the non-randomness of the timing of the treatment have been corrected for, the remaining difference between the probability of nest-leaving before and after paternal retirement can be interpreted as a causal effect of paternal retirement. To account for within family correlation, all standard errors are clustered at the household level.[17]

To estimate model (2.1) using maximum likelihood, I expand the data from a cross-section to a panel dataset by exploiting the retrospective information on the year in which the father retired and his child left home. This means that each individual i ($i = 1, ..., n$) is associated with multiple time periods t_i ($t_i = 1, .., T_{is}$), where T_{is} is the total number of years subject i was at risk for the event.[18] For simplicity of exposition, it is useful to distinguish between the two equations ($j = 1, 2$) because they refer to two different outcomes. For the first equation, age 18 is assumed to be the initial period in which the exposure to the risk of nest-leaving begins,[19] such that t_i goes until the age at which the first event is observed (the child's departure from the parental home). If this event does not occur by the end of the survey, then the child is a right-censored observation and t_i lasts until her age at the time of the interview. A similar reasoning applies to the second

[16] See Imbens and Angrist (1994).

[17] Alternatively, given that eligibility rules vary by country and paternal age, I cluster the standard errors by these two dimensions and find that the results remain virtually unchanged.

[18] This construction follows Jenkins (2005) and Melberg et al. (2010).

[19] This starting age for children is consistent with previous studies (among others, Manacorda and Moretti 2006; Billari and Tabellini 2008; Becker et al. 2010). In my duration analysis, this assumption implies that children under the age of 18 years are left-truncated.

equation, where I now define the father's age when his child is 18 as the onset of risk,[20] thereby allowing t_i to go until either the father's age at which the second event occurs (his retirement) or the father's age at the time of the survey if the father is employed at the end of the observation period (right-censored case). As a result of this reorganization of the data, I obtain an unbalanced panel, as each individual in the two equations is associated with a different number of time units. Furthermore, a new binary dependent variable y_{it} must be created. If individual i is right-censored, then y_{it} is always equal to zero. If individual i is not censored, y_{it} takes value zero for all but the last of $i's$ periods (i.e., year $1, ..., T_{is} - 1$) and takes value 1 in the last period (i.e., year T_{is}). After having experienced the event, the subject no longer contributes to the risk set and is dropped from the sample (right-truncated cases). One issue that arises in this particular setting is the possibility that paternal retirement occurs after children leave the nest. While the majority of my sample is composed of fathers who retire after the departure of their children, these time observations would no longer contribute to explaining the hazard of children's nest-leaving, which is the relevant focus of this study. For this reason, these time units are excluded from the second equation. It is worth stressing that one of the main advantages of the duration analysis over a linear IV setting adopted by previous studies is the allowance for censoring, which leads to the elimination of any constraints on the age at which children left their parents' home. For example, Manacorda and Moretti (2006) limit their analysis to youths aged 18 to 30, whereas Billari and Tabellini (2008) and Becker et al. (2010) focus only on adult children aged up to 35 years old.

Consistent with Melberg et al. (2010), the overall log-likelihood function for the bivariate model (1) depends on both the hazard function and the survival function and is given by:

[20]The vast majority of fathers considered in my sample are at least in their 40s when their child is 18. The rationale for this lower bound is that even fathers in their 40s experience a positive, albeit small, risk of transition into retirement.

$$\mathcal{L} = \sum_{i=1}^{n} \left\{ \sum_{k=1}^{2} \pi_k \left\{ \sum_{j=1}^{2} \left\{ \sum_{t=1}^{T_{i,j}-d_{i,j}} log\left[1-\theta_{j,it}\right] + d_{i,j} log\left[\theta_{j,it}\right] \right\} \right\} \right\} \tag{2.3}$$

where the probabilities π_k represent the proportions of the sample composing each latent class, and $d_{i,j}$ is a dummy variable with a value of 1 if individuals are non-censored and a value of 0 if observations are right-censored. It is worth noting that the likelihood of the non-censored individuals differs from that of the censored ones. For the former group, the likelihood is composed of two elements: the survival function from $t = 1$ to $t = T - 1$ and the hazard function in the last period $t = T$ the subject was exposed to the risk. For the latter group, because the censored individuals are never exposed to the event, the likelihood is given solely by the survival function from $t = 1$ to $t = T$.

To maximize (3) under the presence of unobserved heterogeneity, I follow Melberg et al. (2010) and employ the EM algorithm.[21] This method begins with a vector of parameters, α_0, which includes β_1, β_2, δ, γ, $u = (u_1,u_2)$, and the probability weights, $p = (p_1,\ p_2)$, associated with each of the two latent classes into which my observations may fall. Using these parameters, I create a set of weights for each observation as follows:

$$\pi^0_{k,i} = \frac{p^0_k L^0_{ki}}{\sum_{k=1}^{2} p^0_k L^0_{ki}} \tag{2.4}$$

where $\pi_{k,i}$ represents the probability that individual i is assigned to unobserved heterogeneity group k. Thus, individuals are sorted into the most likely latent class to which they belong, based on their observed outcomes. When probabilities of class membership are estimated, I then construct an expected log-likelihood function, which I maximize over

[21] This is a commonly-used iterative procedure for computing the maximum likelihood estimates when the data are incomplete or have missing values. See, for example, Heckman and Singer (1984) and Ng et al. (1995).

α to obtain α_1. Using α_1, I create a new set of weights, π^1, and repeat the algorithm until convergence.

2.5 Main Results

Before presenting estimates of the model described in the previous section, I provide a visual analysis of the evolution of the estimated hazard functions for nest-leaving and paternal retirement, which are estimated non-parametrically using a kernel-smoothing methodology.[22] In particular, Figure 4 illustrates the pattern of nest-leaving for each European region, with the variable time measured in terms of the number of years since the child turned 18.[23] Overall, this figure notes a number of cross-region differences. These differences include the following: a) in the beginning, in northern Europe, the hazard of nest-leaving for sons and daughters is considerably higher compared to that in the other country regions; b) in all country groups, daughters initially have significantly higher rates of nest-leaving compared to those of sons;[24] c) in southern Europe, there is a proportion of adult children who are at high risk of leaving home even when they are in their 40s, thereby providing further evidence about the prolonged cohabitation of Mediterranean youths in their parents' homes.

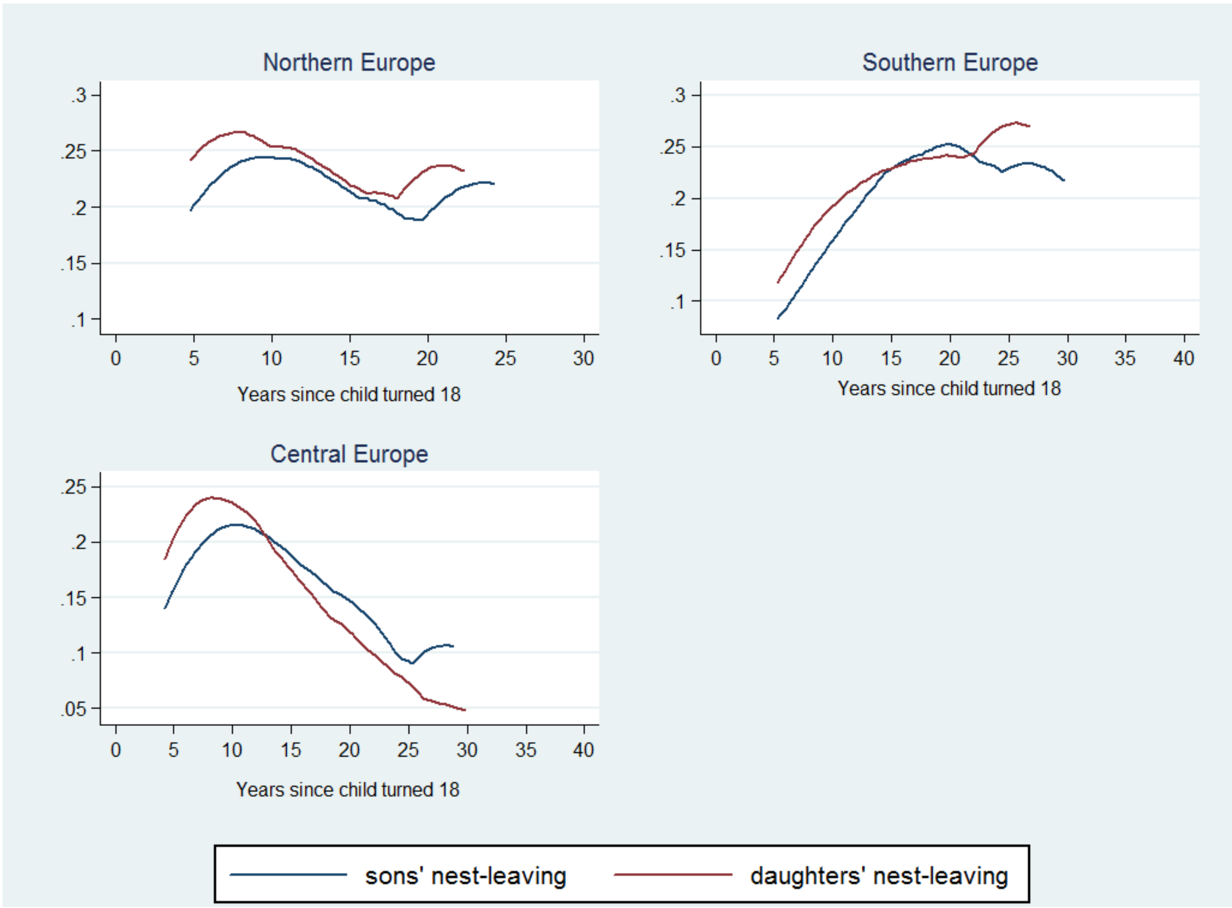
Finally, Figure 5 displays the dynamics of the hazard for paternal retirement. As expected, in all European regions, the hazard of paternal retirement increases with time. It is also evident that fathers living in southern Europe are initially at higher risk of transition into retirement. This result is consistent with the evidence indicating that southern European individuals tend to retire earlier.

[22] This is done using the STS package in STATA. See Cleves et al. (2010) for further details.

[23] Notice that the reason why the smoothed hazard estimate is not depicted for $t < 5$ has to do with the choice of the bandwidth.

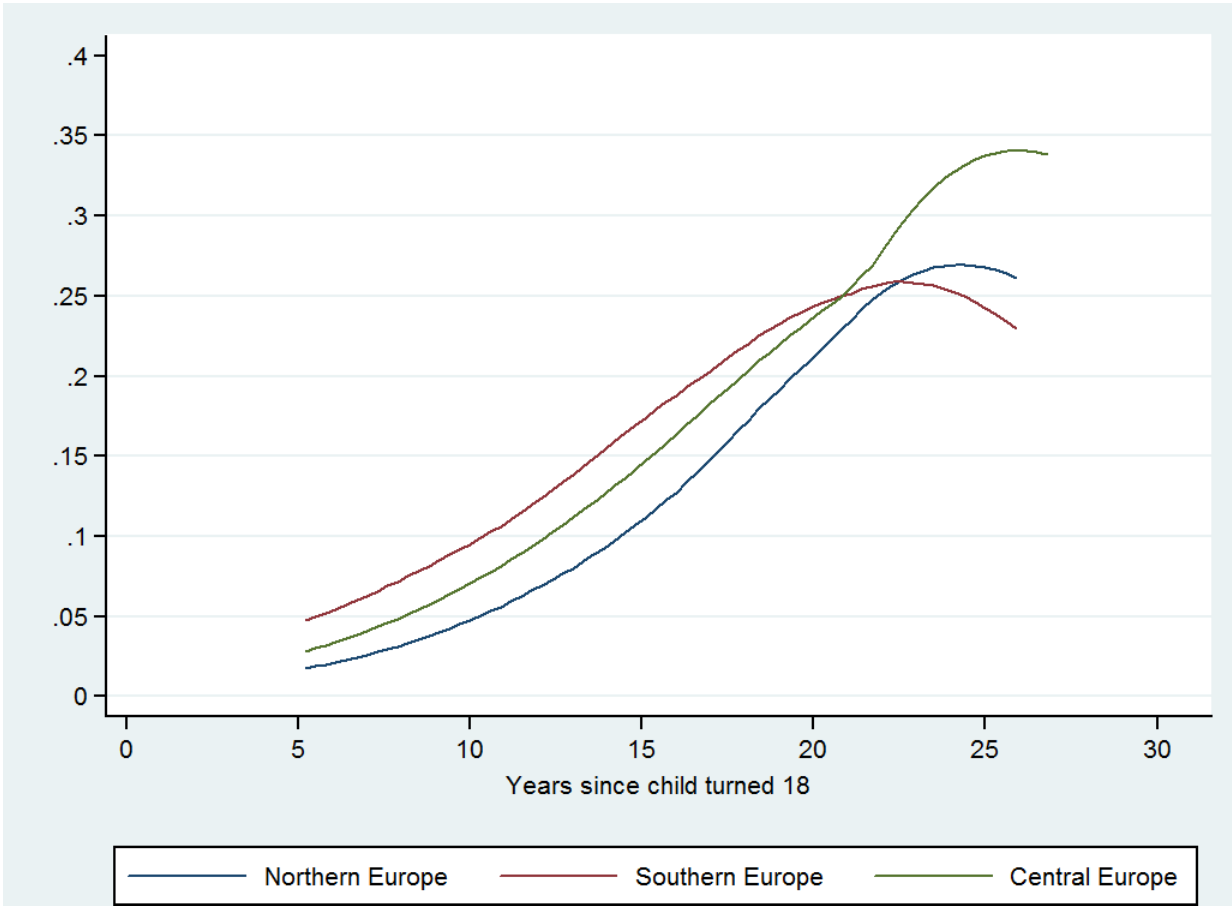
[24] For each country group, the log-rank and Wilcoxon tests clearly reject the null hypothesis that the survivor functions of sons and daughters are the same.

Figure 4: Empirical hazard rate of children's nest-leaving and fathers' retirement, by European region



Notes: This figure plots the estimated hazard function of nest-leaving of children and that of paternal retirement by European region. These hazard functions are estimated using a nonparametric kernel-smoothing methodology (STS package in STATA). Notice that the reason why the smoothed hazard estimate is not depicted for $t < 5$ has to do with the choice of the bandwidth. Recall that children who were less than 18 are left-truncated.

Figure 5: Empirical hazard rate of fathers' retirement, by European region



Notes: This figure plots the estimated hazard function of nest-leaving of children and that of paternal retirement by European region. These hazard functions are estimated using a nonparametric kernel-smoothing methodology (STS package in STATA). Notice that the reason why the smoothed hazard estimate is not depicted for $t < 5$ has to do with the choice of the bandwidth. Recall that children who were less than 18 are left-truncated.

2.5.1 Model without Shared Frailty

I begin by estimating a discrete-time duration model for the hazards of children leaving the nest and paternal retirement without correcting for correlated unobserved heterogeneity. Thus, each equation in model (1) is estimated using a separate logistic hazard equation. Table 4 contains the results, with average marginal effects of covariates on the hazard associated with retirement listed next to their average marginal effects on the hazard of children's nest-leaving. In each specification, I include country fixed effects, cohort fixed effects for fathers and a set of controls such as household size, an indicator for paternal poor health and educational achievement. Specifically, in columns 1, 3 and 5, I estimate the equation explaining the probability of leaving the nest for the first time by dividing the sample into southern, northern and central European countries. When examining southern Europe (see column 1), I find that the estimated effect of paternal retirement is positive and strongly statistically significant (at the 1% level). Paternal retirement implies an increase in the probability of children's nest-leaving of 2.3%. However, when focusing on the northern and central European countries (see columns 3 and 5), the coefficient on paternal retirement becomes insignificant, and the magnitude is reduced to 0.017 and 0.003, respectively. As expected, in each macro-region, the eligibility status for early retirement benefits matters for the hazard of paternal retirement (see columns 2, 4 and 6). While eligible fathers are more likely to retire, the differences in the magnitude of the coefficient on paternal eligibility are remarkable, ranging from 3.2% in northern Europe to 8.9% in southern Europe. In columns 7 and 8, I separately estimate the two equations in model (1) using the pooled sample. Interestingly, the point estimate of the coefficient of interest remains positive and significant, with a magnitude of 0.021. It seems clear that this significant impact on the full sample is driven by the highly significant effects of paternal retirement obtained from the regression on the sample of southern European countries (see column 1). Moreover, I find that coefficients on household size are quite small in magnitude and change signs across the various subsamples for both risks, indicating that household size is not the most important factor for children's nest-leaving or paternal retirement. A similar observation applies to the coefficients on fathers' poor health, which seems to play a very limited role in explaining these two risks. Overall, it is difficult to extrapolate any systematic or interesting patterns from these coefficients.

In sum, although these correlations may suffer from problems of confounding, they provide a first indication that paternal retirement is associated with a higher probability of first nest-leaving by children (first equation) only in the Mediterranean countries, and that early retirement rules strongly predict the hazard of paternal retirement (second equation). In the next subsection, I attempt to establish whether this positive correlation has a causal interpretation.

Table 4: Model without shared frailty - Determinants of the Hazard of Nest-Leaving and Retirement

Sample	**Southern Europe**		**Northern Europe**		**Central Europe**		**Full sample**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome	Nest-leaving	Ret.	Nest-leaving	Ret.	Nest-leaving	Ret.	Nest-leaving	Ret.
Father is retired	0.023***		0.017		0.003		0.021***	
	(0.005)		(0.030)		(0.009)		(0.005)	
Father is eligible		0.089***		0.032***		0.043***		0.055***
		(0.005)		(0.003)		(0.004)		(0.002)
Household size	-0.006**	0.002	0.013***	-0.001	-0.012***	-0.006**	-0.008***	-0.001
	(0.003)	(0.002)	(0.003)	(0.002)	(0.004)	(0.003)	(0.003)	(0.001)
Bad health (father)	0.005	0.004	-0.029***	0.004*	-0.005	0.002	-0.003	0.003*
	(0.004)	(0.004)	(0.010)	(0.003)	(0.006)	(0.003)	(0.003)	(0.002)
Country F.E.	YES	YES	YES	YES	YES	YES	YES	YES
Education F.E.	YES	YES	YES	YES	YES	YES	YES	YES
Cohort F.E.	YES	YES	YES	YES	YES	YES	YES	YES
Birth order F.E.	YES	YES	YES	YES	YES	YES	YES	YES
Log-likelihood	-7,883	-3,185	-6,950	-710	-12,236	-2,298	-27,684	-6,485
Observations	24,530	18,806	13,197	12,597	28,698	23,682	66,425	55,085

Notes: Logit estimations; average marginal effects reported. The sample sizes take into account the longitudinal structure of the data. Education is an indicator for father's college or more (ISCED≥ 5, tertiaty education) and high school education (ISCED=3 or 4, secondary and post-secondary education). Bad health is an indicator that takes value 1 if father's self-reported health is less than good. All specifications include time dummies representing duration dependence. Standard errors in parentheses are clustered at the household level.

* Significant at 10%; ** significant at 5%; *** significant at 1%.

2.5.2 Model with Shared Frailty

The primary concern about the point estimates presented in Table 4 is that they may not adequately account for the correlation between unobserved characteristics that affect children's nest-leaving and unobserved factors that determine paternal retirement, thereby generating omitted variable bias.

To address this concern, I allow for the possibility of correlated unobserved heterogeneity terms across both equations by using the latent class approach adopted by Melberg et al. (2010) and Angelini et al. (2013), in which individuals are divided into two sub-groups of the population. Table 5 presents the estimation results of logistic regressions on the hazard of nest-leaving. As mentioned in the previous subsection, average marginal effects are calculated for each European region (columns 1 to 9) and for the pooled sample (columns 10 to 12). To account for unobservable differences between southern, northern and central Europe, I allow the frailty to vary across these regions. Thus, I separately estimate the probability weights attached to the unobserved heterogeneity Group 1 and Group 2 for each European region as well as for the full sample. The estimated probabilities, $\hat{\pi}_1$ and $\hat{\pi}_2$, are also listed in Table 5.

In particular, in columns 1 to 3, I focus on southern European countries. To facilitate comparisons, in column 1, I report the average marginal effects corresponding to the model in which unobserved heterogeneity is ignored (see, also, column 1 of Table 4). In columns 2 and 3, I present the same predicted effects when unobserved heterogeneity is allowed for by using the probabilities of belonging to Group 1 and Group 2 as weights, respectively. This means that a different logistic hazard regression is estimated for each of the two

Table 5: Model with shared frailty - Determinants of the Hazard of Nest-Leaving, by European region and overall sample

Sample	**Southern Europe**			**Northern Europe**			**Central Europe**			**Full sample**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Unobserved Group	No Het.	Group 1	Group 2	No Het.	Group 1	Group 2	No Het.	Group 1	Group 2	No Het.	Group 1	Group 2
Father is retired	0.023***	0.055***	0.014***	0.017	0.023	-0.097	0.003	0.009	-0.026	0.021***	0.026***	0.002
	(0.005)	(0.007)	(0.005)	(0.030)	(0.025)	(0.067)	(0.009)	(0.009)	(0.021)	(0.005)	(0.007)	(0.005)
Household size	-0.006**	-0.011***	-0.004	0.013***	0.032***	-0.027	-0.012***	-0.011***	-0.001	-0.008***	0.002	-0.010***
	(0.003)	(0.004)	(0.003)	(0.003)	(0.005)	(0.019)	(0.004)	(0.004)	(0.004)	(0.002)	(0.003)	(0.002)
Bad health (father)	0.005	0.015***	0.008*	-0.029***	-0.013	-0.113**	-0.006	-0.007	-0.004	-0.003	0.004	-0.003
	(0.004)	(0.006)	(0.004)	(0.009)	(0.010)	(0.046)	(0.006)	(0.006)	(0.006)	(0.004)	(0.005)	(0.004)
Mass points :												
$\hat{\pi}_1$	0.334			0.065			0.210			0.319		
	(0.325)			(0.196)			(0.290)			(0.336)		
$\hat{\pi}_2$	0.666			0.935			0.790			0.681		
	(0.325)			(0.196)			(0.290)			(0.336)		
Wald test p-value for diff. btw. $\delta^{(2)}$ *and* $\delta^{(3)}$	0.000											
Country F.E.	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Education F.E. (father)	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Cohort F.E. (father)	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Birth order F.E. (child)	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Log-likelihood	-7,883	-2,726	-4,905	-6,950	-5,391	-1,444	-12,236	-9,851	-2,022	-27,684	-8,522	-17,856
Observations	24,530	24,530	24,530	13,197	13,197	10,623	28,698	28,698	22,114	66,425	66,425	57,267

Notes: Logit estimations; average marginal effects reported. $\hat{\pi}_1$ and $\hat{\pi}_2$ are the estimated probability to belong to unobserved heterogeneity Group 1 and Group 2, respectively. The marginal effects are unweighted (col. 1, 4, 7, 10), and weighted, using as weights $\hat{\pi}_1$ (col. 2, 5, 8, 11) or $\hat{\pi}_2$ (col. 3, 6, 9, 12). The sample sizes take into account the longitudinal structure of the data. Education is an indicator for father's college or more (ISCED$\geq$ 5, tertiaty education) and high school education (ISCED=3 or 4, secondary and post-secondary education). Bad health is an indicator that takes value 1 if father's self-reported health is less than good. All specifications include time dummies representing duration dependence. Notice that observations for which the probability of belonging to Group 1 and Group 2 is equal to zero are not included in the sample. Standard errors in parentheses are clustered at the household level.

* Significant at 10%; ** significant at 5%; *** significant at 1%.

groups. The results suggest that paternal retirement is a statistically significant predictor of children's nest-leaving. For those belonging to Group 1, the treatment effect of paternal retirement is positive and strongly statistically significant (at the 1% level). With respect to the magnitude, paternal retirement increases the probability of children's first nest-leaving by 5.5%. The treatment effect remains highly significant, albeit quantitatively less important (1.4%), for those who belong to Group 2.

To learn more about the characteristics of the two groups, Table 6 displays summary statistics on selected covariates.[25] Specifically, individuals in the sample with a predicted probability of falling into Group 1 below the median are assigned to that group, whereas the remaining individuals are placed in Group 2. As evidenced in Panel A (southern Europe), these two groups differ substantially with respect to the proportion of retired fathers. For Group 1, this proportion is approximately 12% greater than the mean of the entire sample (25% versus 22%) and approximately 27% greater than the mean of Group 2 (25% versus 19%). Such significant differences in the fraction of retired fathers can contribute to explaining why young people in Group 1 ("low propensity" nest-leaving types) are much more affected by paternal retirement than their counterparts in Group 2 ("high propensity" nest-leaving types). Interestingly, these two groups also differ significantly in a number of other observable characteristics, such as educational outcomes and children's age at time of leaving home. For instance, adult children in Group 1 are more likely to leave the parental home later and have better outcomes in terms of their own and their fathers' education.

[25] Household size and paternal health status are not shown to save space. However, they are not found to display any significant differences between Group 1 and Group 2.

Table 6: Model with shared frailty - Differences between clusters, by European region and full sample

Variable	**Group 1**		**Group 2**			**Full sample - No Het.**	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean diff. *p-value*	Mean	Std. Dev.
Panel A: Southern Europe ($\hat{\pi}_1 = 0.33, \hat{\pi}_2 = 0.67$)							
Father is retired	0.247	0.431	0.195	0.397	0.000	0.221	0.415
Male (child)	0.570	0.495	0.578	0.494	0.180	0.574	0.495
Married (child)	0.834	0.372	0.831	0.374	0.518	0.833	0.373
High school (father)	0.150	0.357	0.136	0.342	0.001	0.143	0.349
College or more (father)	0.084	0.277	0.073	0.259	0.000	0.078	0.268
High school (child)	0.403	0.490	0.423	0.494	0.001	0.413	0.492
College or more (child)	0.301	0.459	0.235	0.424	0.000	0.268	0.442
Nest-leaving age	30.078	5.268	29.325	5.262	0.000	29.701	5.278
Panel B: Northern Europe ($\hat{\pi}_1 = 0.07, \hat{\pi}_2 = 0.93$)							
Father is retired	0.072	0.259	0.018	0.132	0.000	0.045	0.207
Male (child)	0.610	0.488	0.563	0.496	0.000	0.587	0.492
Married (child)	0.708	0.455	0.678	0.467	0.000	0.693	0.461
High school (father)	0.277	0.448	0.350	0.477	0.000	0.314	0.463
College or more (father)	0.213	0.409	0.282	0.450	0.000	0.247	0.431
High school (child)	0.463	0.499	0.459	0.498	0.656	0.461	0.498
College or more (child)	0.350	0.477	0.388	0.487	0.000	0.369	0.482
Nest-leaving age	26.308	5.196	23.704	4.104	0.000	25.006	4.858

Table 6 (cont.ed): Model with shared frailty - Differences between clusters, by European region and full sample

Variable	**Group 1**		**Group 2**			**Full sample - No Het.**	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean diff. *p-value*	Mean	Std. Dev.
Panel C: Central Europe ($\hat{\pi}_1 = 0.21, \hat{\pi}_2 = 0.79$)							
Father is retired	0.159	0.366	0.040	0.197	0.000	0.100	0.299
Male (child)	0.580	0.494	0.539	0.498	0.000	0.560	0.496
Married (child)	0.706	0.456	0.715	0.451	0.084	0.711	0.453
High school (father)	0.445	0.497	0.429	0.495	0.005	0.437	0.496
College or more (father)	0.272	0.445	0.253	0.435	0.000	0.263	0.440
High school (child)	0.511	0.500	0.456	0.498	0.000	0.483	0.499
College or more (child)	0.430	0.495	0.488	0.500	0.000	0.459	0.498
Nest-leaving age	29.024	7.055	25.326	4.286	0.000	27.175	6.122
Panel D: Full sample ($\hat{\pi}_1 = 0.32, \hat{\pi}_2 = 0.68$)							
Father is retired	0.172	0.377	0.123	0.328	0.000	0.147	0.354
Male (child)	0.574	0.495	0.561	0.496	0.000	0.567	0.495
Married (child)	0.724	0.447	0.779	0.415	0.000	0.751	0.432
High school (father)	0.334	0.472	0.277	0.448	0.000	0.306	0.460
College or more (father)	0.217	0.412	0.164	0.370	0.000	0.190	0.393
High school (child)	0.469	0.499	0.438	0.496	0.000	0.453	0.498
College or more (child)	0.392	0.488	0.351	0.477	0.000	0.371	0.483
Nest-leaving age	28.560	6.299	26.807	5.172	0.000	27.684	5.829

When restricting the analysis to northern Europe (columns 4 to 6 of Table 5) and central Europe (columns 7 to 9 of Table 5), I find that the dummy variable for paternal retirement is no longer statistically significant in any of the two unobserved groups. This lack of significance can likely be explained by looking at the differences in the fraction of adult children who left the nest after paternal retirement. Table 3 reveals that such differences across European regions are enormous, ranging from 42% in southern Europe to 15% in central Europe and to 6% in northern Europe. In other words, when fathers retire, only a very limited share of adult offspring in northern and central European countries is still living with their parents, thus raising concerns about the lack of power in my identification strategy for these two macro-regions.

Descriptive statistics (see Panel B for northern Europe and Panel C for central Europe) confirm that young people in Group 1 can still be viewed as "low propensity" nest-leaving types, with a much larger fraction of retired fathers. To be more precise, in northern and central Europe, these fractions are approximately 60% higher compared to the mean of the full sample, and they are four times larger when they are compared to the mean of the respective Group 2. Moreover, in northern and central Europe, young people belonging to Group 1 tend to leave the nest later relative to their counterparts in Group 2.

In columns 10 to 12 of Table 5, I report the estimated coefficients obtained from the pooled sample. While treatment effects of paternal retirement are positive and significant for Group 1, they are close to zero for Group 2. Similar to the analysis ignoring unobserved heterogeneity (see column 7 in Table 4), it seems evident that the significant effect for Group 1 on the pooled sample is driven by the strongly significant effect obtained for the same group in southern Europe. As expected, when examining the descriptive statistics (see Panel D in Table 6), individuals in Group 1 are characterized by a markedly larger share of retired fathers compared to those belonging to Group 2 (17% higher with respect to the mean of the full sample and 40% higher relative to the mean of Group 2) and are more likely to leave the nest later. It is also worth noting that the estimated probability of belonging to Group 1 varies substantially with the associated macro-region and is much higher in southern Europe (33%) as opposed to northern (6%) and central (21%) Europe. This result confirms that young people sharing some latent characteristics that make them belong to the latent class of “late” nest-leavers (Group 1) are concentrated in southern European countries. Overall, the evidence presented above suggests that, although quantitatively small, there are positive causal effects of paternal retirement on the timing of children’s nest-leaving only for southern European countries. The non-significant effects obtained for northern and central Europe are presumably because most youths have already left their parental homes at the time of their fathers’ retirement. In the discussion section, I explain why these findings may differ so largely by European region.

Moreover, Table 7 presents the estimates for the hazard of paternal retirement. In accordance with the model in which unobserved heterogeneity is not allowed for (see Table 4), the coefficients on eligibility status reveal the significant influence of eligibiliy rules on actual retirement. These findings are consistent with the available empirical evidence on the relevance of early retirement incentives (Gruber and Wise 2004). Interestingly, in the southern European countries, the strength of the estimated effects is larger compared to that of the other country groups. This may be because Italian, Spanish and Greek workers have more financial incentives to retire early due to their particularly generous early retirement benefits with respect to those of other European regions.

Finally, in an attempt to disentangle the treatment effects of paternal retirement on sons from the effects on daughters in southern Europe, I separately consider the samples of male and female children. The results for sons and daughters are presented in Table 8. When restricting the analysis to sons (see columns 2 and 3), the coefficient on paternal retirement varies between 5.5% for individuals in Group 1 and 1.3% for those belonging to Group 2. A similar pattern is observed in the regressions for daughters (see columns 5 and 6), with the difference being that the magnitude for daughters in Group 1 is slightly smaller compared

Table 7: Model with shared frailty - Determinants of the Hazard of Retirement, by European region and overall sample

Sample	**Southern Europe**			**Northern Europe**			**Central Europe**			**Full sample**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Unobserved Group	No Het.	Group 1	Group 2	No Het.	Group 1	Group 2	No Het.	Group 1	Group 2	No Het.	Group 1	Group 2
Father is eligible	0.089***	0.100***	0.087***	0.032***	0.039***	0.023***	0.043***	0.047***	0.037***	0.055***	0.065***	0.061***
	(0.005)	(0.000)	(0.005)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.002)	(0.003)	(0.003)
Household size	0.002	-0.009	0.011***	-0.001	-0.000	0.004	-0.006**	-0.009**	0.003***	-0.001	0.001	-0.004***
	(0.002)	(0.000)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.001)	(0.001)	(0.002)	(0.001)
Bad health (father)	0.004	-0.025	0.019***	0.004*	0.004	0.002	0.002	0.001	0.008***	0.003*	0.005**	0.005***
	(0.004)	(0.000)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
Mass points :												
$\hat{\pi}_1$	0.334			0.065			0.210			0.319		
	(0.325)			(0.196)			(0.290)			(0.336)		
$\hat{\pi}_2$	0.666			0.935			0.790			0.681		
	(0.325)			(0.196)			(0.290)			(0.336)		
Country F.E.	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Education F.E. (father)	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Cohort F.E. (father)	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Birth order F.E. (child)	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Log-likelihood	-3,185	-892	-2,080	-710	-618	-319	-2,298	-2073	-110	-6,485	-1,777	-4,186
Observations	18,806	18,806	18,806	12,597	12,597	12,597	23,682	23,682	18,419	55,085	55,085	49,822

Notes: Logit estimations; average marginal effects reported. $\hat{\pi}_1$ and $\hat{\pi}_2$ are the estimated probability to belong to unobserved heterogeneity Group 1 and Group 2, respectively. The marginal effects are unweighted (col. 1, 4, 7, 10), and weighted, using as weights $\hat{\pi}_1$ (col. 2, 5, 8, 11) or $\hat{\pi}_2$ (col. 3, 6, 9, 12). The sample sizes take into account the longitudinal structure of the data. Education is an indicator for father's college or more (ISCED$\geq$ 5, tertiaty education) and high school education (ISCED=3 or 4, secondary and post-secondary education). Bad health is an indicator that takes value 1 if father's self-reported health is less than good. All specifications include time dummies representing duration dependence. Notice that observations for which the probability of belonging to Group 1 and Group 2 is equal to zero are not included in the sample. Standard errors in parentheses are clustered at the household level.

* Significant at 10%; ** significant at 5%; *** significant at 1%.

to sons in Group 1 (4.9% vs. 5.5%) and the treatment effect for daughters in Group 2 is no longer significant, which may be partly due to the smaller sample size.. However, these differences between sons and daughters are not significantly different from zero. In Tables A1 and A2 in Appendix A, I show that paternal retirement has no significant positive effects on sons and daughters in northern and central Europe.

Table 8: Model with shared frailty - Hazard of Nest-Leaving in Southern Europe, Sons and Daughters

Sample	**Sons**			**Daughters**		
	(1)	(2)	(3)	(4)	(5)	(6)
Unobserved Group	No Het.	Group 1	Group 2	No Het.	Group 1	Group 2
Father is retired	0.024***	0.055***	0.013**	0.017**	0.049***	0.011
	(0.006)	(0.009)	(0.007)	(0.008)	(0.011)	(0.008)
Household size	-0.009***	-0.014***	-0.006*	-0.001	-0.004	-0.002
	(0.003)	(0.005)	(0.003)	(0.005)	(0.007)	(0.005)
Bad health (father)	0.007	0.020***	0.011**	0.004	0.009	0.005
	(0.005)	(0.007)	(0.006)	(0.007)	(0.010)	(0.007)
Mass points :						
$\hat{\pi}_1$	0.334			0.334		
	(0.325)			(0.325)		
$\hat{\pi}_2$	0.666			0.666		
	(0.325)			(0.325)		
Wald test p-value for diff. btw. $\delta^{(2)}$ *and* $\delta^{(3)}$	0.000					
Wald test p-value for diff. btw. $\delta^{(5)}$ *and* $\delta^{(6)}$	0.00					
Country F.E.	YES	YES	YES	YES	YES	YES
Education F.E. (father)	YES	YES	YES	YES	YES	YES
Cohort F.E. (father)	YES	YES	YES	YES	YES	YES
Birth order F.E. (child)	YES	YES	YES	YES	YES	YES
Log-likelihood	-4,115	-1,431	-2,529	-3,672	-1,255	-2,304
Observations	14,076	14,076	14,076	10,454	10,454	10,454

Notes: * Significant at 10%; ** significant at 5%; *** significant at 1%.

2.6 Sensitivity Analysis

In this section, I perform a variety of robustness checks to test how the results change when I modify the estimation strategy or use a different specification of the model (see Tables 9 and 10).

Table 9: Effects of paternal retirement, IV analysis

	(1)	(2)	(3)	(4)
Sample	South	North	Central	Overall
Panel A: 2SLS				
Dep. Var.: Child leaves home				
Father is retired	0.159**	-0.253	-0.046	0.042
	(0.075)	(0.235)	(0.066)	(0.066)
Household size	-0.007**	-0.022***	-0.033***	-0.022***
	(0.003)	(0.004)	(0.011)	(0.007)
Bad health (father)	0.014**	0.000	0.014	0.010
	(0.007)	(0.012)	(0.012)	(0.008)
Observations	34,462	37,135	54,976	126,573
R^2	0.223	0.201	0.221	0.258
First stage F statistic	82.06	9.12	98.99	159.68
Panel B: First stage				
Dep. Var.: Father is retired				
Father is eligible	0.442***	0.132*	0.246***	0.454***
	(0.020)	(0.044)	(0.025)	(0.009)
Household size	0.005	-0.003	-0.017***	-0.001
	(0.004)	(0.007)	(0.005)	(0.006)
Bad health (father)	0.046***	0.033*	0.028***	0.027***
	(0.005)	(0.010)	(0.008)	(0.007)
Observations	34,462	37,135	54,976	126,573
R^2	0.175	0.188	0.214	0.202

Notes: * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table 10: Sensitivity of Estimates

Sample	Southern Europe			Northern Europe			Central Europe			Full sample		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Unobserved Group	No Het.	Group 1	Group 2	No Het.	Group 1	Group 2	No Het.	Group 1	Group 2	No Het.	Group 1	Group 2
Panel A: Baseline												
Father is retired	0.023***	0.055***	0.014***	0.017	0.023	-0.097	0.003	0.009	-0.026	0.021***	0.026***	0.002
	(0.005)	(0.007)	(0.005)	(0.030)	(0.025)	(0.067)	(0.009)	(0.009)	(0.021)	(0.005)	(0.007)	(0.005)
Mass points :												
$\hat{\pi}_1$	0.334			0.065			0.210			0.319		
	(0.325)			(0.196)			(0.290)			(0.336)		
$\hat{\pi}_2$	0.666			0.935			0.790			0.681		
	(0.325)			(0.196)			(0.290)			(0.336)		
Log-likelihood	-7,883	-2,726	-4,905	-6,950	-5,391	-1,444	-12,236	-9,851	-2,022	-27,684	-8,522	-17,856
Observations	24,530	24,530	24,530	13,197	13,197	10,623	28,698	28,698	22,114	66,425	66,425	57,267
Panel B: Gateway Effect - 3 years												
Father is retired	0.020***	0.038***	0.011*	0.017	0.021	-0.055	0.004	0.009	-0.011	0.021***	0.028***	0.001
	(0.006)	(0.009)	(0.007)	(0.023)	(0.020)	(0.114)	(0.011)	(0.011)	(0.031)	(0.006)	(0.009)	(0.007)
Mass points :												
$\hat{\pi}_1$	0.334			0.065			0.210			0.319		
	(0.325)			(0.196)			(0.290)			(0.336)		
$\hat{\pi}_2$	0.666			0.935			0.790			0.681		
	(0.325)			(0.196)			(0.290)			(0.336)		
Log-likelihood	-4,599	-1,866	-2,711	-4,242	-210	-3,872	-8,058	-2,165	-5,467	-17,088	-7,489	-8,709
Observations	24,530	24,530	24,530	13,197	13,197	10,623	28,698	28,698	22,114	66,425	66,425	57,267

Notes: Logit estimations; average marginal effects reported. $\hat{\pi}_1$ and $\hat{\pi}_2$ are the estimated probability to belong to unobserved heterogeneity Group 1 and Group 2, respectively. All specifications include controls for paternal education, country dummies, and birth cohort dummies for fathers (in 1-year interval). All specifications include time dummies representing duration dependence. Notice that observations for which the probability of belonging to Group 1 and Group 2 is equal to zero are not included in the sample. Standard errors in parentheses are clustered at the household level.

* Significant at 10%; ** significant at 5%; *** significant at 1%.

2.6.1 Instrumental Variable Analysis

Although the bivariate hazard model described in section 4 provides the most appropriate description of the relationship between paternal retirement and the timing of children's nest-leaving, there may still be concerns regarding the sensitivity of my results to their stability or to the parametric assumptions made in the estimation. As noted by Melberg et al. (2010), in latent class models, the convergence of the likelihood can be vulnerable to problems due to local optima. To address this concern, I estimate the following linear version of model (1) using two stage least squares (2SLS):

$$Pr(L_{it} = 1) = \alpha + \beta Retired_{it} + \gamma X_i + \epsilon_{it} \tag{2.5}$$

where the treatment dummy $Retired_{it}$ and the variable X_i are defined in the same way as in Section 4. Here, the outcome variable L_{it} is a dummy taking the value 1 if a child i residing in a given country left the parental home at age t. Following Manacorda and Moretti (2006), I focus on youth aged 18 to 30 years.[26] Finally, ϵ_{it}^{f} represents an idiosyncratic error term, which is presumably correlated with the outcome variable because it embodies unobserved factors of fathers, including ability, which might affect children's home-leaving decisions. Consistent with previous analysis, I would expect to find a positive and significant effect of paternal retirement only in southern Europe.

I identify the causal effect of paternal retirement on children's nest-leaving using cross-country changes in eligibility rules for early retirement benefits for the period 1961 to 2007 as an instrument for paternal retirement. As discussed in Section 4, this instrument is recognized to be relevant and arguably exogenous to children's living arrangements. In

[26] Alternatively, consistent with Billari and Tabellini (2008), I consider children aged 18 to 35, obtaining similar results. The results are available from the author upon request.

this setup, the first stage regression is given by:

$$Retired_{it} = \delta_0 + \delta_1 Eligibility_{it} + \pi X_i + \nu_{it} \tag{2.6}$$

where the dummy $Eligibility_{it}$ represents the instrument introduced in Section 4. As previously mentioned, it is important to acknowledge that this instrumental variable strategy is relevant only for the subpopulation of individuals who retire as a consequence of early retirement schemes.

Panel A of Table 9 reports the 2SLS results. The treatment dummy on paternal retirement is positive and significant at the 5% level only for southern Europe (see column 1). This dummy variable, however, becomes non-significant and negative for northern and central European countries (see columns 2 and 3). Panel B contains the first-stage results. As expected, these estimates indicate that eligibility for early retirement benefits is an important determinant for paternal retirement. Altogether, the IV analysis lends some additional evidence that for southern Europe there is a positive causal relation between paternal retirement and children's nest-leaving, a finding that calls for further explanation.

2.6.2 Additional Sensitivity Checks

As a further check, I investigate the robustness of my estimates to the use of an alternative definition of the treatment dummy for paternal retirement. A common concern is that as children age, they are more likely to leave the parental home regardless of their fathers' retirement status. To address this concern, I define a time frame of three years, and construct a binary variable that is set to 1 if the father retired prior to the child's first move-out within the time frame[27] and 0 otherwise. This approach is similar in spirit to that of van Ours (2003), who refers to this time frame as the "incubation period" to identify a gateway effect of cannabis on cocaine. The results are presented in Panel B of

[27] I also check the sensitivity of the estimates to time frames of 2 and 4 years and obtain similar results.

Table 10. Reassuringly, these parameter estimates resemble those obtained in the benchmark specification, with the only difference being that in Southern Europe the magnitude of the estimated effects of paternal retirement becomes slightly smaller.

2.7 Discussion

In the literature on moving-out decisions, what remains largely unexplained is the mechanism regulating the positive causal relationship between paternal retirement and children's nest-leaving. In this section, I start to fill this gap by focusing the analysis on Italy, Greece and Spain, countries for which I found a positive causal effect of paternal retirement.[28] A unique feature of these southern European countries is that they can be divided into two groups. One group is composed of Italy and Greece, where there is a large bonus payment at the time of retirement that amounts to approximately three times the gross annual salary. The second group includes only Spain, where such severance payment does not exist.[29] My information on severance arrangements is drawn from Holzmann et al. (2011), from personal communications with national experts and from other country-specific sources.[30] As previously mentioned, the literature would attribute this causal relationship mainly to two competing mechanisms. To provide an empirical test for these two mechanisms, I adopt a differences-in-differences strategy, where Italy and Greece constitute the treatment group and Spain is the control group, "unaffected" by the lump-sum payment upon retirement. The key identification assumption for Spain to be a valid control group is that children's nest-leaving behavior of Spain and Italy and Greece would have followed similar trends over time, in the absence of retirement severance pay. It is plausible to justify this assumption, given that, conditional on country fixed effects,

[28] Unfortunately, SHARE data does not contain information regarding the reason for children's nest-leaving.

[29] As noted by Garcia-Gomez et al. (2013), Spanish employed that leave employment and transit into unemployment may receive a severance payment from the employer. To overcome this issue, I excluded from the analysis individuals who declare themselves as retired because they were made redundant. However, in Table A6 in Appendix A, I show that the main conclusions are not affected by including these individuals.

[30] For Italy, information on retirement severance payment is obtained from Miniaci et al. (2003). For Greece and Spain, institutional details have been integrated by personal communications with Samuel Bentolila, Olympia Bover, Pilar Garcia-Gomez, Athanasios Tagkalakis and Platon Tinios.

the southern European countries included in my sample were undergoing similar economic conditions and were very similar in terms of welfare state regime, family structure and culture.

To the extent that the Manacorda and Moretti mechanism is at play, I expect paternal retirement to bribe Italians and Greek adult children to stay at home longer as a consequence of the positive shock to the family's liquidity associated with the retirement severance payment. However, the results reported in Table 11 (columns 1 to 3) are in the opposite direction. For individuals belonging to Groups 1 and 2, the dummy variable for paternal retirement remains positive and highly statistically significant (at the 1% level), with a magnitude of 6.1% and 1.5%, respectively. This result indicates that liquidity problems faced by fathers at the time of retirement do not provide an entirely satisfactory explanation. On the other hand, if retirement severance payment mattered, as stressed by Battistin et al. (2009), I would expect to find no evidence of significant effects of paternal retirement for Spain. Nevertheless, the coefficient estimates presented in columns 4 to 6 largely contradict the prediction of this second hypothesis: for individuals in Group 1, the estimated coefficient on paternal retirement retains its significance, whereas for those in Group 2, the magnitude of the coefficient of interest remains substantially unchanged with respect to the estimate in column 3, but is significant only at the 10% level. This result is what I expected given the reduced sample size.

Table 11: Potential mechanisms: Manacorda and Moretti (2006) vs. Battistin et al. (2009) hypotheses

Sample	**Italy and Greece**			**Spain**		
	(1)	(2)	(3)	(4)	(5)	(6)
Unobserved Group	No Het.	Group 1	Group 2	No Het.	Group 1	Group 2
Father is retired	0.024***	0.061***	0.015**	0.031***	0.049***	0.020*
	(0.006)	(0.009)	(0.006)	(0.011)	(0.016)	(0.012)
Household size	-0.005	-0.007	-0.005*	-0.011**	-0.019***	-0.005
	(0.003)	(0.006)	(0.003)	(0.005)	(0.006)	(0.006)
Bad health (father)	0.005	0.020***	0.005	0.010	0.007	0.012
	(0.005)	(0.008)	(0.005)	(0.009)	(0.009)	(0.010)
Mass points :						
$\hat{\pi}_1$	0.334			0.334		
	(0.325)			(0.325)		
$\hat{\pi}_2$	0.666			0.666		
	(0.325)			(0.325)		
Country F.E.	YES	YES	YES	YES	YES	YES
Education F.E. (father)	YES	YES	YES	YES	YES	YES
Cohort F.E. (father)	YES	YES	YES	YES	YES	YES
Birth order F.E. (child)	YES	YES	YES	YES	YES	YES
Log-likelihood	-5,508	-1,942	-3,388	-2,337	-767	-1,501
Observations	16,960	16,960	16,960	6,820	6,820	6,820

Notes: * Significant at 10%; ** significant at 5%; *** significant at 1%.

The main conclusion that I draw from this empirical test is that the decline in children's cohabitation at paternal retirement cannot be entirely ascribed to liquidity problems or a boost in family's income due to severance payment.

One may still be concerned that Spain is not a comparable control group or that Italy and Greece do not represent an appropriate treatment group because self-employed workers are not entitled to retirement severance payment. In order to address these concerns, I propose an additional test: for Italy and Greece, I use the employed as the treatment and self-employed[31] as the control group. Descriptive statistics in Table 12 demonstrate that employed and self-employed do not differ significantly in a large number of observable characteristics, thus providing support for the claim that self-employed workers are a valid counterfactual. The results reported in Table 13 indicate that there are positive causal effects of paternal retirement on the timing of children's nest-leaving for the treatment (columns 1 to 3) and control group (columns 4 to 6), which I interpret as corroborating evidence that the drop in paternal post-retirement income or retirement severance payment do not provide a satisfactory explanation for the mechanism behind children's nest-leaving upon paternal retirement.

[31]Self-employed refer to those individuals who have been self-employed at any stage during their career.

Table 12: Summary Statistics, Employed vs. Self-employed

Variable	Employed			Self-employed			
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Mean diff. *p-value*
Age (father)	689	69.869	7.199	240	70.222	6.723	0.534
Household size	689	2.334	0.653	240	2.320	0.718	0.799
Retired	689	0.932	0.252	240	0.872	0.335	0.006
Retirement age	642	58.555	4.719	209	61.701	4.287	0.000
Bad health	689	0.412	0.493	240	0.325	0.470	0.026
High school (father)	689	0.192	0.394	240	0.123	0.329	0.024
College or more (father)	689	0.075	0.264	240	0.044	0.206	0.123
High school (child)	689	0.492	0.500	240	0.463	0.500	0.469
College or more (child)	689	0.266	0.442	240	0.227	0.420	0.255
Nest-leaving age (child)	689	27.145	5.121	240	26.931	5.139	0.601
Married (child)	689	0.774	0.419	240	0.818	0.387	0.180

Notes: This table reports the observations from the cross-sectional sample before reshaping it as a longitudinal dataset. All the samples contain individuals for whom information on children's nest-leaving age and paternal education is not missing and exclude children who were less than 18. The paternal sample consists of all individuals who are either working or retired.

Table 13: Potential mechanisms: Employed vs. Self-employed in Italy and Greece

Sample	**Employed**			**Self-employed**		
	(1)	(2)	(3)	(4)	(5)	(6)
Unobserved Group	No Het.	Group 1	Group 2	No Het.	Group 1	Group 2
Father is retired	0.018***	0.057***	0.011*	0.037***	0.053***	0.028*
	(0.007)	(0.010)	(0.007)	(0.012)	(0.018)	(0.015)
Household size	-0.009**	-0.005	-0.007	0.003	-0.005	0.001
	(0.004)	(0.006)	(0.005)	(0.007)	(0.011)	(0.009)
Bad health (father)	0.007	0.030***	0.003	-0.005	-0.013	0.010
	(0.006)	(0.008)	(0.006)	(0.012)	(0.020)	(0.013)
$Mass\ points$:						
$\hat{\pi}_1$	0.334			0.334		
	(0.325)			(0.325)		
$\hat{\pi}_2$	0.666			0.666		
	(0.325)			(0.325)		
Country F.E.	YES	YES	YES	YES	YES	YES
Education F.E. (father)	YES	YES	YES	YES	YES	YES
Cohort F.E. (father)	YES	YES	YES	YES	YES	YES
Birth order F.E. (child)	YES	YES	YES	YES	YES	YES
Log-likelihood	-5,508	-1,942	-3,388	-2,337	-767	-1,501
Observations	12,901	12,901	12,901	4,059	4,059	4,059

Notes: Logit estimations; average marginal effects reported. $\hat{\pi}_1$ and $\hat{\pi}_2$ are the estimated probability to belong to unobserved heterogeneity Group 1 and Group 2, respectively. The marginal effects are unweighted (col. 1, 4), and weighted, using as weights $\hat{\pi}_1$ (col. 2, 5) or $\hat{\pi}_2$ (col. 3, 6). The sample sizes take into account the longitudinal structure of the data. Education is an indicator for father's college or more (ISCED$\geq$ 5, tertiaty education) and high school education (ISCED=3 or 4, secondary and post-secondary education). Bad health is an indicator that takes value 1 if father's self-reported health is less than good. Notice that observations for which the probability of belonging to Group 1 and Group 2 is equal to zero are not included in the sample. Standard errors in parentheses are clustered at the household level.

* Significant at 10%; ** significant at 5%; *** significant at 1%.

For this reason, it seems worthy to investigate other potential channels. In their study on the intergenerational effects of Italian pension reforms on fertility, Battistin et al. (2013) argue that the rise in retirement age has reduced the amount of informal child care provided by grandparents, which in turn has determined an increase in the children's age at first child and of home leaving. While this scenario can be applied to other southern European countries, including Spain and Greece, in the present study the negative shock to the provision of informal child care is not likely to be a major determinant for the prolonged cohabitation with parents. It is in fact well-known that grandmothers are the main provider of informal child care arrangements to their grandchildren (see, for instance, Richter et al. 1994). A similar result is also obtained in Battistin et al. (2013), who show that it is the grandmothers' provision of informal child care that plays a primary role in their children's fertility decisions, finding that an additional grandmother at home raises her daughter's probability of being mother by 4%. However, as argued previously, female partners are excluded from my analysis. I also provide a more formal test to address the concern that individuals in a couple may plan their retirement closely together (Stancanelli 2012). In Table 14, I show that, even when focusing only on fathers whose spouses have never worked, there still exists a positive and quantitatively similar causal effect of paternal retirement on the hazard of children's nest-leaving. It may be argued, however, that some evidence in favor of this interpretation cannot be totally ruled out given that for those in Group 2 the coefficient on paternal retirement is close to zero, thus revealing the potential presence of an effect originating from the grandparents' supply of informal child care on top of other unexplained factors for the "early" nest-leaving types.

Table 14: Potential mechanisms: Fathers whose wives never worked

Sample	**Southern Europe**		
	(1)	(2)	(3)
Unobserved Group	No Het.	Group 1	Group 2
Father is retired	0.016**	0.054***	0.005
	(0.008)	(0.011)	(0.008)
Household size	-0.016***	-0.026***	-0.015**
	(0.006)	(0.008)	(0.006)
Bad health (father)	0.004	0.013	0.010
	(0.007)	(0.010)	(0.008)
Mass points :			
$\hat{\pi}_1$	0.334		
	(0.325)		
$\hat{\pi}_2$	0.666		
	(0.325)		
Country F.E.	YES	YES	YES
Education F.E. (father)	YES	YES	YES
Cohort F.E. (father)	YES	YES	YES
Birth order F.E. (child)	YES	YES	YES
Log-likelihood	-7,883	-2,726	-4,905
Observations	9,435	9,435	9,435

Notes: * Significant at 10%; ** significant at 5%; *** significant at 1%.

It is therefore worthwhile to investigate a number of preference-related reasons why children's coresidence may decline immediately after paternal retirement.

Although it is not a contribution of this paper, it remains to be explored why the coefficient on paternal is not statistically significant in northern and central Europe. As argued in Section 5, a plausible explanation is that there is not enough power in my identification strategy for these two macro-regions because only a very limited share of adult offspring left their parental home after paternal retirement. However, this finding raises the issue of why young people living in northern and central Europe leave home much earlier relative to their counterparts in southern Europe. Such disparities in the age of home-leaving can be reconciled with cross-regional differences in housing markets (Alessie et al. 2006), family ties (Alesina and Giuliano 2011) and labor market conditions (Card and Lemieux 2000).

2.8 Conclusion

This study examines the relationship between paternal retirement and the timing of housing emancipation of young adults in Europe, with the aim of testing empirically which of the mechanisms proposed in the literature dominates in practice. Taking advantage of the retrospective dimension of my micro data, I use a bivariate discrete hazard model with shared frailty and exploit cross-country variation in early retirement legislation. Overall, my regression results suggest that there is a significant influence of paternal retirement on the probability of first nest-leaving of children living in Southern European countries. However, there is no evidence of significant effects on children residing in Northern and Central European countries. I interpret this evidence as indicating that paternal retirement is a relevant explanatory variable of coresidence decisions only in Southern Europe, once differences in institutions, culture and other unobservables are controlled for.

To shed some light into the mechanism, I provide an empirical test for the two main competing channels by which paternal retirement may be thought to affect children's nest-leaving. Comparing my cross-country evidence for Southern Europe with important country-specific evidence obtained for Italy from two other studies (Manacorda and Moretti

2006; Battistin et al. 2009), it seems plausible to conclude that the decline in children's co-residence around paternal retirement does not seem to be driven by changes in parents' budget constraints. In addition, I test the plausibility of the argument proposed by Battistin et al. (2013); however, I do not find conclusive evidence that the supply of informal child care provided by grandparents is a major determinant for children's moving-out. Rather, one needs to look for channels involving negative externalities in preferences between parents and children.

Empirical evidence that paternal retirement can affect children's nest-leaving has important policy implications. It is well-known that because the population is rapidly aging in Europe, it is becoming increasingly important to maintain the long-term financial sustainability of pension systems. To achieve this goal, in the recent past European governments have primarily adopted a number of pension reforms that have raised the retirement age or removed financial incentives to early retirement. However, the results of this paper suggest that in Southern Europe policy makers should also be aware that there may be potential unintended and undesirable consequences of pension reforms on moving-out decisions of young people.

References

Aassve, A., F. C. Billari, S. Mazzucco, and F. Ongaro (2002). "Leaving home: A comparative analysis of ECHP data." *Journal of European Social Policy*, 12, 259-275.

Abbring. Jaap H., and Gerard J. van den Berg (2003). "The nonparametric identification of treatment effects in duration models" *Econometrica,* vol. 71, No. 5, pp. 1491-1517.

Abbring. Jaap H., and Gerard J. van den Berg (2005). "The effect of Unemployment Insurance Sanctions on the Transition Rate from Unemployment to Employment." *Economic Journal*, 115(505): 602-630.

Albertini, Marco, and Martin Kohli (2012). "The Generational Contract in the Family: An Analysis of Transfer Regimes in Europe" *European Sociological Review*, 0(0): 1-13.

Albertini, Marco, Martin Kohli, and Claudia Vogel (2007). "Intergenerational transfers of time and money in European families: common patterns - different regimes?" *Journal of European Social Policy*, 17(4): 319-334.

Alesina, Alberto, and Paola Giuliano (2010). "The power of the family" *Journal of Economic Growth*, 15(2): 93-125.

Alessie, Rob, Agar Brugiavini, and Guglielmo Weber (2006). "Saving and cohabitation: the economic consequences of living with one's parents in Italy and The Netherlands" In R.H. Clarida, J.A. Frankel, F. Giavazzi, & K.D. West (Eds.), *NBER international seminar on macroeconomics* 2004 (pp. 413-441). Cambridge, MA: MIT Press.

Angelini, Viola, Agar Brugiavini, and Guglielmo Weber (2009). "Ageing and unused capacity in Europe: is there an early retirement trap?" *Economic Policy*, pp. 463-508.

Angelini, Viola, Bucciol Alessandro, Wakefield Matthew, and Guglielmo Weber (2013). "Can Temptation Explain Housing Choices in Later Life?"

Attanasio, Orazio P., and Agar Brugiavini (2003). "Social Security and Households' Saving." *The Quarterly Journal of Economics,* 118(3), 1075-1119.

Battistin, Erich, Agar Brugiavini, Enrico Rettore, and Guglielmo Weber (2009). "The Retirement Consumption Puzzle: Evidence from a Regression Discontinuity Approach" *American Economic Review*, vol. 99(5), pp. 2209-2226.

Battistin, Erich, Michele De Nadai, and Mario Padula (2013). "Unintended Consequences of Pension Reforms on Inter-Generational Fertility" *Working Paper.*

Becker, Sascha, Samuel Bentolila, Ana Fernandes, and Andrea Ichino (2010). "Youth emancipation and perceived job insecurity of parents and children." *Journal of Population Economics*, 23, 1047-1071.

Billari, Francesco, and Guido Tabellini (2010). "Italians are late: Does it matter?" In J.B. Shoven (Ed.), *Demography and the economy*, NBER Books (pp. 371-412). Chicago: University of Chicago Press.

Billari, F. C., D. Philipov, and P. Baizan (2001). “Leaving home in Europe: The experience of cohorts born around 1960.” *International Journal of Population Geography*, 7, 339-356.

Card, David, and Thomas Lemieux (2000). “Adapting to Circumstances: The Evolution of Work, School, and Living Arrangements Among North American Youth.” In Youth Employment and Joblessness in Advanced Countries, edited by D. Blanchflower and R. Freeman. University of Chicago Press.

Cleves, Mario, Roberto G. Gutierrez, William Gould, and Yulia V. Marchenko (2010). “An Introduction to Survival Analysis Using Stata.” *Stata Press.*

Duval, Romain (2003). “The retirement effects of old-age pension and early retirement schemes in OECD countries” OECD.

Garcia-Gomez, Pilar, Sergi Jimenez-Martin, and Judit Vall Castello (2013). “Financial incentives, health and retirement in Spain”, fedea Working Paper.

Garrouste, Christelle, and Omar Paccagnella (2010). “Data Quality: Three Examples of Consistency Across SHARE and SHARELIFE”, in *SHARELIFE Methodology.* Mannheim Research Institute for the Economics of Aging (MEA).

Gibson, John, and Bonggeun Kim (2005). “Can we trust retrospective recall to replace panel data?”

Giuliano, Paola (2007). “Living arrangements in Central Europe: Does cultural origin matter?” *Journal of the European Economic Association*, 5(5), 927-952.

Gruber, J., and Wise D. (2004). “Social Security Programs and Retirement around the World: Micro-Estimation.” *University of Chicago Press,* Chicago.

Havari, Enkelejda, and Fabrizio Mazzonna (2011). “Can We trust older people’s statements on their childhood circumstances? Evidence from SHARELIFE.” SHARE Working Paper 05/2011.

Heckman, James J. and Burton Singer (1984). “A Method for Minimizing the Impact of Distributional Assumptions in Econometric Models for Duration Data.” *Econometrica* 52(2): 271-320.

Holzmann, Robert, Yann Pouget, Milan Vodopivec, and Michael Weber (2011). “Severance Pay Programs around the World: History, Rationale, Status, and Reforms” *IZA Discussion Paper No. 5731.*

Imbens, GW, Angrist JD (1994). “Identification and Estimation of Local Average Treatment Effects” *Econometrica* 62(2): 467-475.

Jenkins, P. Stephen (2005). “Survival Analysis”, *https://www.iser.essex.ac.uk/resources/survival-analysis-with-stata*

Liebman, Jeffrey B., Erzo F.P. Luttmer, and David G. Seif (2009). “Labor Supply Responses to Marginal Social Security Benefits: Evidence from Discontinuities” *Journal of Public Economics* 93(11-12), 1208-1223.

Manacorda, Marco, and Enrico Moretti (2006). “Why do most Italian youths live with their parents? Intergenerational transfers and household structure” *Journal of the European Economic Association* 4(4):800-829.

Melberg, Hans Olav, Andrew M. Jones, and Anne Line Bretteville-Jensen (2010). “Is cannabis a gateway to hard drugs?” *Empirical Economics,* 38:583-603.

Miniaci, Raffaele, Chiara Monfardini, and Guglielmo Weber (2003). “Is There a Retirement Consumption Puzzle in Italy?” *Institute for Fiscal Studies Working Paper W03/14.*

Ng SK, Krishnan T, and GJ McLachlan (1995). “The EM algorithm” *In: Gentle JE, Härdle W, Mori Y (eds) Handbook of computational statistics, Springer, Heidelberg, pp. 137-168.*

Pudney, Stephen (2003). “The road to ruin? Sequences of initiation to drug use and crime in Britain” *Economic Journal*, 113:182-198.

Reher, David Sven (1998). “Family Ties in Central Europe: Persistent Contrasts.” *Population and Development Review,* 24:203-34.

Richter, Kerry, Chai Podhisita, Apichat Chamratrithirong, and Kusol Soonthorndhada (1994). “The impact of child care on fertility in urban Thailand.” *Demography,* 31:651:662.

Salisbury, Laura (2012). “Women’s Income and Marriage Markets in the United States: Evidence from the Civil War Pension” *IED Discussion Paper Series,* n. 244.

Stancanelli, Elena (2012). “Spouses’ Retirement and Hours Oucomes: Evidence from Twofold Regression Discontinuity with Differences-in-Differences” *IZA Discussion Paper,* n. 6791.

van Ours, JC (2003). “Is Cannabis a stepping-stone for cocaine?” *Journal of Health Economics,* 22:539-554.

Vaupel, J.W., K. Manton and E. Stallard (1979). “The impact of heterogeneity in individual frailty on the dynamics of mortality.” *Demography* 16: 439-54.

Appendix A: Supplemental Tables

Table A1: Determinants of the Hazard of Nest-Leaving in Northern Europe, Sons and Daughters

Sample		**Sons**			**Daughters**	
	(1)	(2)	(3)	(4)	(5)	(6)
Unobserved Group	No Het.	Group 1	Group 2	No Het.	Group 1	Group 2
Father is retired	0.028	0.029	-0.099	-0.005	0.012	-0.100
	(0.038)	(0.027)	(0.078)	(0.020)	(0.036)	(0.088)
Household size	0.012	0.033***	-0.037	0.008	0.028*	-0.032
	(0.013)	(0.004)	(0.027)	(0.010)	(0.016)	(0.025)
Bad health (father)	-0.014	-0.002	-0.071**	-0.050***	-0.029***	-0.162***
	(0.010)	(0.010)	(0.036)	(0.009)	(0.009)	(0.044)
Mass points :						
$\hat{\pi}_1$	0.065			0.065		
	(0.196)			(0.196)		
$\hat{\pi}_2$	0.935			0.935		
	(0.196)			(0.196)		
Country F.E.	YES	YES	YES	YES	YES	YES
Education F.E. (father)	YES	YES	YES	YES	YES	YES
Cohort F.E. (father)	YES	YES	YES	YES	YES	YES
Birth order F.E. (child)	YES	YES	YES	YES	YES	YES
Log-likelihood	-3,837	-2,984	-785	-3,050	-2,360	-634
Observations	7,740	7,740	6,105	5,453	5,453	4,508

Notes: * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table A2: Determinants of the Hazard of Nest-Leaving in Central Europe, Sons and Daughters

Sample	**Sons**			**Daughters**		
	(1)	(2)	(3)	(4)	(5)	(6)
Unobserved Group	No Het.	Group 1	Group 2	No Het.	Group 1	Group 2
Father is retired	0.012	0.014	0.010	-0.012	-0.005	-0.058
	(0.011)	(0.011)	(0.031)	(0.014)	(0.014)	(0.036)
Household size	-0.017***	-0.016**	-0.007	-0.011**	-0.010*	0.000
	(0.006)	(0.007)	(0.007)	(0.006)	(0.005)	(0.006)
Bad health (father)	-0.004	-0.002	-0.010	-0.009	-0.013	0.004
	(0.007)	(0.007)	(0.009)	(0.009)	(0.009)	(0.010)
Mass points :						
$\hat{\pi}_1$	0.210			0.210		
	(0.290)			(0.290)		
$\hat{\pi}_2$	0.790			0.790		
	(0.290)			(0.290)		
Country F.E.	YES	YES	YES	YES	YES	YES
Education F.E. (father)	YES	YES	YES	YES	YES	YES
Cohort F.E. (father)	YES	YES	YES	YES	YES	YES
Birth order F.E, (child)	YES	YES	YES	YES	YES	YES
Log-likelihood	-6,380	-5,011	-1,038	-5,760	-4,489	-950
Observations	16,069	16,069	12,062	12,629	12,629	10,052

Notes: * Significant at 10%; ** significant at 5%; *** significant at 1%.

Table A3: Differences between clusters, Sons and Daughters in Southern Europe

Variable	**Group 1**		**Group 2**			**Full sample - No Het.**	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean diff. *p-value*	Mean	Std. Dev.
Panel A: Sons ($\hat{\pi}_1 = 0.33, \hat{\pi}_2 = 0.67$)							
Father is retired	0.253	0.435	0.203	0.402	0.000	0.228	0.419
Married (child)	0.812	0.391	0.792	0.406	0.003	0.802	0.398
High school (father)	0.136	0.343	0.142	0.349	0.310	0.139	0.345
College or more (father)	0.073	0.260	0.063	0.244	0.020	0.068	0.252
High school (child)	0.396	0.489	0.397	0.489	0.913	0.397	0.489
College or more (child)	0.255	0.436	0.230	0.421	0.000	0.242	0.428
Nest-leaving age	30.635	5.127	30.120	5.139	0.000	30.377	5.139
Panel B: Daughters ($\hat{\pi}_1 = 0.33, \hat{\pi}_2 = 0.67$)							
Father is retired	0.243	0.429	0.183	0.386	0.000	0.213	0.409
Married (child)	0.867	0.340	0.883	0.322	0.313	0.875	0.330
High school (father)	0.168	0.374	0.128	0.335	0.000	0.148	0.355
College or more (father)	0.099	0.299	0.084	0.278	0.000	0.092	0.288
High school (child)	0.410	0.492	0.460	0.498	0.000	0.435	0.495
College or more (child)	0.362	0.481	0.243	0.429	0.009	0.302	0.459
Nest-leaving age	29.352	5.368	28.237	5.228	0.045	28.794	5.327

Notes: Descriptive statistics are computed using the longitudinal sample. $\hat{\pi}_1$ and $\hat{\pi}_2$ are the estimated probability to belong to unobserved heterogeneity Group 1 and Group 2, respectively. Observations with an estimated probability below the median are assigned to Group 1, whereas the remaining individuals are assigned to Group 2.

Table A4: Differences between clusters, Sons and Daughters in Northern Europe

Variable	Group 1		Group 2			Full sample - No Het.	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean diff. *p-value*	Mean	Std. Dev.
Panel A: Sons ($\hat{\pi}_1 = 0.07, \hat{\pi}_2 = 0.93$)							
Father is retired	0.080	0.272	0.020	0.140	0.000	0.050	0.218
Married (child)	0.700	0.458	0.659	0.474	0.000	0.679	0.466
High school (father)	0.302	0.459	0.335	0.472	0.001	0.318	0.465
College or more (father)	0.179	0.383	0.275	0.447	0.000	0.227	0.419
High school (child)	0.475	0.499	0.438	0.496	0.668	0.457	0.498
College or more (child)	0.331	0.471	0.384	0.486	0.000	0.357	0.479
Nest-leaving age	26.981	5.288	24.175	4.251	0.000	25.578	4.997
Panel B: Daughters ($\hat{\pi}_1 = 0.07, \hat{\pi}_2 = 0.93$)							
Father is retired	0.059	0.235	0.016	0.125	0.000	0.037	0.189
Married (child)	0.723	0.448	0.702	0.458	0.092	0.712	0.452
High school (father)	0.235	0.424	0.380	0.485	0.000	0.307	0.461
College or more (father)	0.260	0.439	0.291	0.454	0.009	0.276	0.446
High school (child)	0.450	0.498	0.485	0.500	0.009	0.467	0.498
College or more (child)	0.370	0.483	0.402	0.490	0.012	0.386	0.486
Nest-leaving age	25.346	4.949	23.040	3.734	0.000	24.193	4.532

Notes: Descriptive statistics are computed using the longitudinal sample. $\hat{\pi}_1$ and $\hat{\pi}_2$ are the estimated probability to belong to unobserved heterogeneity Group 1 and Group 2, respectively. Observations with an estimated probability below the median are assigned to Group 1, whereas the remaining individuals are assigned to Group 2.

Table A5: Differences between clusters, Sons and Daughters in Central Europe

Variable	Group 1		Group 2			Full sample - No Het.	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean diff. *p-value*	Mean	Std. Dev.
Panel A: Sons ($\hat{\pi}_1 = 0.21, \hat{\pi}_2 = 0.79$)							
Father is retired	0.156	0.363	0.053	0.225	0.000	0.105	0.305
Married (child)	0.690	0.463	0.696	0.460	0.360	0.693	0.461
High school (father)	0.443	0.497	0.446	0.497	0.718	0.444	0.496
College or more (father)	0.289	0.454	0.238	0.426	0.000	0.264	0.440
High school (child)	0.513	0.500	0.468	0.499	0.000	0.491	0.499
College or more (child)	0.429	0.495	0.462	0.499	0.000	0.446	0.497
Nest-leaving age	29.595	6.959	25.980	4.715	0.000	27.787	6.211
Panel B: Daughters ($\hat{\pi}_1 = 0.21, \hat{\pi}_2 = 0.79$)							
Father is retired	0.156	0.363	0.031	0.173	0.000	0.093	0.290
Married (child)	0.727	0.446	0.739	0.439	0.124	0.733	0.442
High school (father)	0.440	0.496	0.416	0.493	0.006	0.428	0.494
College or more (father)	0.260	0.438	0.264	0.441	0.595	0.262	0.439
High school (child)	0.504	0.500	0.444	0.497	0.000	0.474	0.499
College or more (child)	0.440	0.496	0.513	0.500	0.000	0.477	0.499
Nest-leaving age	28.211	7.106	24.590	3.613	0.000	26.400	5.917

Notes: Descriptive statistics are computed using the longitudinal sample. $\hat{\pi}_1$ and $\hat{\pi}_2$ are the estimated probability to belong to unobserved heterogeneity Group 1 and Group 2, respectively. Observations with an estimated probability below the median are assigned to Group 1, whereas the remaining individuals are assigned to Group 2.

Table A6: Potential mechanisms: Manacorda and Moretti (2006) vs. Battistin et al. (2009) hypotheses

Sample	**Italy and Greece**			**Spain**		
	(1)	(2)	(3)	(4)	(5)	(6)
Unobserved Group	No Het.	Group 1	Group 2	No Het.	Group 1	Group 2
Father is retired	0.024***	0.061***	0.015**	0.025***	0.047***	0.015
	(0.006)	(0.009)	(0.006)	(0.010)	(0.015)	(0.011)
Household size	-0.005	-0.007	-0.005*	-0.008*	-0.017***	-0.004
	(0.003)	(0.006)	(0.003)	(0.005)	(0.006)	(0.006)
Bad health (father)	0.005	0.020***	0.005	0.009	0.008	0.014
	(0.005)	(0.008)	(0.005)	(0.008)	(0.009)	(0.009)
$Mass\ points$:						
$\hat{\pi}_1$	0.334			0.334		
	(0.325)			(0.325)		
$\hat{\pi}_2$	0.666			0.666		
	(0.325)			(0.325)		
Country F.E.	YES	YES	YES	YES	YES	YES
Education F.E. (father)	YES	YES	YES	YES	YES	YES
Cohort F.E. (father)	YES	YES	YES	YES	YES	YES
Birth order F.E. (child)	YES	YES	YES	YES	YES	YES
Log-likelihood	-5,508	-1,942	-3,388	-2,337	-767	-1,501
Observations	16,960	16,960	16,960	7,570	7,570	7,570

Notes:* Significant at 10%; ** significant at 5%; *** significant at 1%.

Chapter 3

The Effect of Divorce Risk on the Wealth and Retirement Security of Households

Abstract: This paper investigates the effect of an increase in the risk of divorce on the wealth and financial retirement security of married couples. My empirical strategy exploits changes in divorce laws occurred between the late 1970s and the 2000s in Europe as an exogenous shock to the risk of marital breakup. Across countries and over time, these laws shifted the ground for divorce from mutual consent to unilateral choice. This "divorce revolution" allows for the use of a quasi-experimental design that exploits the time and country variation in these laws to identify the empirical relationship between divorce risk and the wealth and retirement security of households. By employing a unique dataset that contains detailed information on the wealth and the marital history of the couple, I discuss and test the potential mechanisms by which the divorce legislation affects the retirement well-being of married couples. Overall, my results lend support to the precautionary motive for saving. The increase in the risk of divorce leads to a significant asset accumulation of married couples around retirement.

3.1 Introduction

Over the last few years, a substantial body of research has investigated the effects of unilateral divorce laws on a large array of household outcomes, such as divorce rates (Friedberg 1998; Wolfers 2006), the welfare of children (Gruber 2004), marital conflict (Stevenson and Wolfers 2006) and women's labor supply decisions (Gray 1998; Stevenson 2007). However, little attention to date has been given to the potential effects of divorce laws on dimensions related to the retirement standard of living of elderly people. In this paper, I seek to fill this gap by exploring the causal impact of an increase in divorce risk on the wealth accumulation of elderly people in Europe. This empirical relationship can be identified by the quasi-natural experiment provided by the wave of liberal divorce reforms that took place in Europe during the last four decades of the 20th century. Across countries and over time, these laws made divorce less difficult: the legal regime switched from "fault divorce" to "no-fault divorce" and in most cases the grounds for divorce shifted from mutual agreement to unilateral choice. Evidence from European countries shows that the shift to unilateral divorce regime was accompanied by a rapid rise in divorce rates, with an average increase of about 0.3-0.4 annual divorces per 1000 people (González and Viitanen 2009). This relationship has also generated a great deal of attention from policy makers because it lies at the heart of the public debate over the retirement security of the elderly population. There are increasing concerns that many baby boomers are approaching retirement with low levels of financial wealth and virtually no assets other than their homes (Lusardi and Mitchell 2007). This is a particular concern for women who tend to live longer than men, have less attachment to labor force, earn less and are more financially illiterate.

Some recent theoretical papers emphasize that changes in the likelihood of divorce may affect the incentive to save of households; however, researchers are still puzzled about the possible mechanisms underlying this relationship. There are two main competing explanations through which an increase in the risk of marital breakup may influence households saving behavior. On the one hand, Cubeddu and Rios-Rull (1997) show that increases in marital dissolution encourage household saving. They attribute this rise in saving to standard precautionary motives: since households cannot insure themselves against divorce, a greater risk of marital dissolution induce married couples to save more. On the other hand, Mazzocco et al. (2007) stress that an increase in the probability of divorce would make saving while married more risky because asset division laws tend to distribute resources equally or equitably, thereby creating incentives to increase current consumption. This pattern is particularly relevant for marriages in which spouses face a high probability of divorce. Only a few contributions, however, have made an attempt to empirically test which of the channels dominates in practice. Voena (2013) develops a model to evaluate the impact of divorce laws and different property rights regimes on the intertemporal labor supply and saving behavior of married couples. She tests the model using US panel data and provides support for the precautionary saving channel by showing that when the risk of marital breakup increases and assets are equally divided among spouses, men tend to increase savings to offset the possible loss of half of their assets to wives if divorce occurs. Similarly, Gonzalez and Ozcan (2013), using Irish panel data, find that the shift from mutual consent to unilateral divorce leads to a significant increase in the propensity to save by married individuals. The positive relationship between risk of family disruption and households asset accumulation, however, is not entirely consistent with the work by Stevenson (2007), which provides evidence of a decline in the propensity to undertake marriage-specific investments, such as supporting a spouse through school or buying a home, with the relation varying with the property division regime prevailing in the US states.

Overall, these studies do not consider the retirement well-being of married couples as the outcome and limit their analyses to specific countries (US and Ireland). Therefore, a key emphasis on retirement security and a cross-country analysis allows this study to address questions that other researchers have not. To the best of my knowledge, this is the first paper to estimate the impact of different divorce law reforms on the retirement security of married couples and evaluate the relative weight of the two competing explanations using a European dataset. To achieve identification, I take advantage of an exogenous increase in the risk of divorce provided by the unilateral divorce legislation in Europe and estimate a fixed-effects OLS regression to identify its effect on household retirement well-being.

3.2 Data and Institutional Context

In my empirical analysis, I use data from the the Survey of Health, Ageing and Retirement in Europe (SHARE). This survey collects detailed information on demographics, current socio-economic status, health, expectations as well as social and family networks for nationally representative samples of European individuals aged fifty and over who speak the official language of each country, and do not live abroad or in an institution, plus their spouses or partners irrespective of age. The main advantage of this data source is related to the representativeness of the sample of elderly people in Europe because this survey is constructed to ensure comparability of the analysis across the different countries. In this study, I use data from the third wave collected in 2008/2009. This wave, known as SHARELIFE, focuses on many aspects of life histories of respondents and it is particularly suitable for my investigation, since it provides uniquely detailed data on individuals' marital histories. In this paper, I present evidence from eleven countries for which I was able to collect information on unilateral divorce laws. These countries represent the various regions of continental Europe, ranging from Scandinavia (Sweden and Denmark) through Central Europe (Austria, Belgium, France, Germany, Switzerland and the Netherlands) and the Mediterranean countries (Italy, Spain and Greece).

In my sample selection, because economic outcomes are measured at the household level, I constrain the sample to couples who are still in their first marriage at the time of the interview. Consistent with Alessie et al. (2013), I restrict my attention to spouses born between 1931 and 1952. The choice of this interval allows me to consider all the couples, aged 55-75 in the interview year of wave 2, who married before or after the introduction of unilateral divorce in their corresponding country. I also use data from the second wave of SHARE because information on economic well-being later in life were not collected in SHARELIFE. These indicators include total net worth, financial assets, real assets and home-ownership. To be more precise, the total net worth is computed as the sum of the net values of: (a) the primary residence net of mortgage; (b) other real estate; (c) business; (d) cars; (e) savings, stocks and bonds, mutual funds, IRA's and life insurances. Imputations on net worth are provided in the SHARE data to correct for missing values. This net worth is expressed in PPP-adjusted Euros. Home-ownership is measured as a dichotomous variable that takes value 0 if a respondent does not own the house he or she occupies at the time of the interview and 1 if he or she does.

As regards the institutional context, I use data on unilateral divorce laws across the above-mentioned European countries, building on the work by Viitanen (2009) and Kneip et al. (2009). The table below illustrates the year of introduction of de-facto unilateral divorce laws and the distribution of the sample of couples married before and after the unilateral divorce laws across countries. A feature worth stressing is that there is a large multi-country variability in the year of introduction of unilateral divorce laws. Especially striking is that the year of introduction ranges from 1915 in Sweden to 2000 in Switzerland and 2010 in Italy. The distribution of total net worth and other wealth components appears in Table 2. The financial wealth distribution is very skewed: households' median financial wealth is €18,607, while the mean is approximately three times greater (€51,766). Table 3 shows the distribution of total net worth and home ownership by educational groups as well as for couples without children.

Table 1: De-Facto Unilateral Divorce Laws by Country, Sample of Married Households 1931-1952

Country	Year of introduction	No unilateral divorce at time first marriage	Unilateral divorce at time first marriage	Total
Austria	1978	190	28	218
Belgium	1975	633	167	800
Denmark	1970	264	292	556
France	1976	459	143	602
Germany (FRG)	1977	372	83	455
Greece	1983	823	86	909
Italy	2010	963	0	963
Netherlands	1971	380	292	672
Spain	1981	587	53	640
Sweden	1915	0	454	454
Switzerland	2000	340	1	341
Total		5,011	1,599	6,610

Notes: Source: González et al. (2009), Kneip et al. (2009), Reinhold et al. (2012). Column 2 shows the year when de facto unilateral, no-fault divorce was first allowed.

Table 2: Distribution of total net worth and wealth components, Sample of Households (€2007)

Percentile	Total net worth (€)	Housing wealth (€)	Financial wealth (€)
p5	9,599	1,924	-1,368
p10	34,902	8,928	0
p25	116,375	93,738	2,553
p50	222,097	184,330	18,607
p75	381,705	303,004	64,028
p90	558,648	462,815	158,238
p95	682,896	575,941	227,752
Mean	270,333	218,568	51,766
Std dev	209,533	178,492	80,916

Table 3: Total net worth and home ownership by demographic group, Sample of Households (€2007)

Group	25th pctl	Median	Mean	75th pctl	N	% Owner first house	% Owner second house	N
Education								
<HS	86,509	166,527	208,366	293,364	2,730	0.82	0.24	2,500
HS graduate	127,292	236,395	272,263	384,953	1,996	0.81	0.24	1,850
Some college	166,588	272,035	313,267	417,797	244	0.82	0.41	225
College graduate	191,894	326,757	362,748	495,397	1,603	0.87	0.34	1,483
>College	268,986	363,765	460,118	602,399	40	0.89	0.46	35
Children								
None	103,923	214,921	261,748	363,248	378	0.78	0.23	340
Some	116,660	222,732	270,853	382,775	6,247	0.83	0.27	5,765

3.3 Empirical Model

To examine how an increase in the risk of divorce generated by the shift to unilateral divorce laws affects the retirement well-being of married couples, I estimate the following fixed-effects OLS regression:

$$\begin{aligned} retirement\ wealth_{ij} &= \beta_0 + \beta_1 Degree\ of exposure_{ij} + \gamma X_i + \sum_j Country\ fixed\ effects_j + \\ & \sum_i Year\ of\ marriage\ fixed\ effects_i + \sum_c Cohort\ fixed\ effects_c + \epsilon_{ij} \end{aligned}$$

where the dependent variable, $retirement\ wealth_{ij}$, is a measure of retirement preparedness for household i living in country j, i.e., the total net worth, financial assets, the total value of non-housing assets and home ownership. It is important to include a measure for retirement wealth. This inclusion will be done in a subsequent version of the paper. It is worth stressing that all economic variables are defined at the time of the household interview, which rules out the possibility of using a differences-in-differences approach. $Degree\ of\ exposure_{ij}$ measures the exposure intensity, i.e., the ratio between the numbers of years the household was exposed to unilateral divorce and the marriage duration. The rationale for using this continuous variable is that a couple might have married before the law change but may have spent most of their marrriage during the period in which the unilateral regime was in place. Formally, the variable can be written in the following way:

$$Degree\ of\ exposure_{ij} = \begin{cases} (years\ of\ exposure)_{ij}/(years\ since\ marriage)_i & if\ couple\ i\ married\ before\ UDL \\ 1 & if\ couple\ i\ married\ after\ UDL \end{cases}$$

Therefore, I construct the variable of interest in such a way that it depends on three factors: the country j in which the reform took place, the couple's year of marriage, and the year when unilateral divorce law was first allowed. The coefficient of main interest β_1 identifies whether the retirement wealth is larger for households for whom divorce becomes less costly at the time of first marriage. Equation (1) then includes a set of country, year of marriage and cohort dummies. X is a vector of household-level controls, such as household size and education. Finally, ϵ_{ij} represents an idiosyncratic error term.

I identify the causal effect of the risk of divorce on the wealth of married couples around retirement by exploiting the variation provided by divorce laws over couple's year of marriage and across European countries. The key identification assumption is that changes in divorce laws generate an exogenous increase in the risk of marital dissolution. In other words, these laws provide a source of variation in divorce risk that is credibly exogenous and unlikely to be related to unobservable characteristics of spouses that might explain the different wealth accumulation behaviors around retirement. While it seems plausible to argue that the timing of divorce laws came as a surprise to married couples, one may still be concerned that in some cases the precise timing of divorce laws may not occur randomly. The main threat to identification is that couples may be able to anticipate when the unilateral divorce laws will become effective, and in response to this expected event, they may modify their year of marriage. As a result, the anticipation of unilateral divorce laws by couples would violate the identifying assumption described above, thereby producing biased estimates. In order to test for this possibility, in the robustness analysis I exclude spouses who married in the close vicinity of the divorce laws and show that my results remain substantially unchanged.

3.4 Results

Table 4 and Table 5 present the estimation results of main interest. In column 1 of Table 4 I find that a one standard deviation increase in the exposure to divorce laws induce couples to accumulate more assets during retirement. The magnitude of the effects is large: a one standard deviation increases wealth accumulation by approximately 20% for total wealth (see column 1 of Table 4), 53% for financial wealth (see column 2 of Table 4) and 18% for real wealth (see column 3 of Table 4).

Table 4: Naive OLS - logs - Sample of Married Couples

Dependent Variable:	(1) Total wealth - logs	(2) Financial wealth - logs	(3) Real wealth - logs
Degree of exposure	0.196**	0.528***	0.179*
	(0.085)	(0.103)	(0.094)
Country F.E.	YES	YES	YES
Cohort F.E. (for each spouse)	YES	YES	YES
Years married (quadratic)	YES	YES	YES
Family size	YES	YES	YES
Observations	6,134	5,229	6,055
R^2	0.061	0.203	0.064

Notes: Standard errors in parentheses are clustered by household's country and year of marriage. The degree of exposure is defined as the ratio between the years of exposure to UDL and the marriage duration. The dependent variable is in logs. Degree of exposure is standardized. *** p<0.01, ** p<0.05, * p<0.1.

Table 5: Naive OLS - levels - Sample of Married Couples

Dependent Variable:	(1) Total wealth - levels	(2) Financial wealth - levels	(3) Real wealth - levels
Degree of exposure	112,245***	37,813***	74,432**
	(37,753)	(12,346)	(35,083)
Country F.E.	YES	YES	YES
Cohort F.E. (for each spouse)	YES	YES	YES
Years married (quadratic)	YES	YES	YES
Family size	YES	YES	YES
Observations	6,191	6,191	6,191
R^2	0.076	0.188	0.065

Notes: Standard errors in parentheses are clustered by household's country and year of marriage. The degree of exposure is defined as the ratio between the years of exposure to UDL and the marriage duration. The dependent variable is in levels. *** p<0.01, ** p<0.05, * p<0.1.

3.5 Conclusion

The risk of family disruption may have potential effects on several important dimensions related to household well-being. In this paper, I examine how the risk of divorce affects the wealth accumulation of elderly people, exploiting divorce laws in Europe. Overall, the results indicate that the shift from mutual agreement to unilateral divorce determine a significant accumulation of wealth by married couples around retirement, which offers support to the standard precautionary motive for saving.

References

Alessie, Rob, Viola Angelini, and Peter van Santen (2013). "Pension wealth and household savings in Europe: Evidence from SHARELIFE" *European Economic Review*.

Cubeddu, Luis, and Jose V. Rios-Rull (1997). "Marital Risk and Capital Accumulation." Staff Report 235, Federal Reserve Bank of Minneapolis.

Dewilde, Caroline, Karel Van den Bosch, and Aaron Van den Heede (2010). "Separation: Consequences for Wealth in Later Life." In A. Börsch-Supan, M. Brandt, K. Hank, and M. Schröder (Eds.), *The individual and the welfare state, life histories in Europe* (pp. 103-114). Heidelberg: Springer.

Friedberg, Leora (1998). "Did Unilateral Divorce Raise Divorce Rates? Evidence from Panel Data." *American Economic Review*, Vol. 88(3):608-627.

González, Libertad, and Tarja K. Viitanen (2009). "The effect of divorce laws on divorce rates in Europe" *European Economic Review*, Vol. 53: 127-138

González, Libertad, and Berkay Ozcan (2013). "The risk of divorce and Household Saving Behavior" *Journal of Human Resources*, Vol. 48: 404-434

Gray, Jeffrey (1998). "Divorce-law Changes, Household Bargaining and Female Labor Supply." *American Economic Review*, Vol. 88(3): 628-642.

Gruber, Jonathan (2004). "Is making divorce easier bad for children? The long-run implications of unilateral divorce." *Journal of Labor Economics*, Vol. 22(4): 799-833.

Kneip, Thorsten, and Gerrit Bauer (2009), "Did unilateral divorce laws raise divorce rates in Western Europe?" *Journal of Marriage and Family*, Vol. 71, pp. 592-607.

Lusardi, Annamaria, and Olivia S. Mitchell (2008). "Planning and Financial Literacy: How Do Women Fare?" *American Economic Review*, Vol. 98(2): 413-417.

Mazzocco, Maurizio, Claudia Ruiz, and Shintaro Yamaguchi (2007). "Labor Supply, Wealth Dynamics, and Marriage Decisions." UCLA. Unpublished.

Munnell, Alicia (2004). "Why Are So Many Older Women Poor?" *Just the Facts 10*, Center for Retirement Research.

Reinhold, Steffen, Thorsten Kneip, and Gerrit Bauer (2012). "The long run consequences of unilateral divorce laws on children - evidence from SHARELIFE" *Journal of Population Economics*

Stevenson, Betsey (2007). "The Impact of Divorce Laws on Marriage-Specific Capital" *Journal of Labor Economics*, Vol. 25: 75-94

Stevenson, Betsey and Justin Wolfers (2006). "Bargaining in the Shadow of the Law: Divorce Laws and Family Distress" *Quarterly Journal of Economics*, vol. 121(1): 267-288.

Stevenson, Betsey and Justin Wolfers (2007). "Marriage and divorce: Changes and their driving forces." *Journal of Economic Perspectives*, Vol. 21(2): 27-52.

Voena, Alessandra (2011). "Yours, Mine and Ours: Do Divorce Laws Affect the Intertemporal Behavior of Married Couples?" Job Market Paper.

Wolfers, Justin (2006). "Did Unilateral Divorce Laws Raise Divorce Rates? A Reconciliation and New Results." *American Economic Review*, Vol. 96(5):1802-1820.